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# American SOCIOLOGICAL Review



Acceptance of Industrialization and Its Attendant Consequences for the  
Social Patterns of Non-Western Societies *George A. Theodorson*

The Eurasian Minority in Indonesia *Justus M. van der Kroef*

An Investigation of the Distinctiveness of Social Classes  
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Predicting Marital Success or Failure in an Urban Population  
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Vol. 18

October 1953

No. 5

Official Journal of the American Sociological Society

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# American SOCIOLOGICAL Review

October  
1953

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Official Journal of the American Sociological Society

## ACCEPTANCE OF INDUSTRIALIZATION AND ITS ATTEND- ANT CONSEQUENCES FOR THE SOCIAL PATTERNS OF NON-WESTERN SOCIETIES

GEORGE A. THEODORSON

Cornell University

IT is the main thesis of this paper that the industrialization of non-machine societies will eventually lead to the development of new societal patterns.<sup>1</sup> These patterns will resemble, in time, certain dominant patterns of western industrialized society, which *may not be rejected by any people who accept the machines of the West.*

The view that only very limited aspects of western culture can be imported into the non-machine societies while certain other "less desirable" aspects can be excluded is naive, unless it is based on an understanding of those aspects of culture which are independent of the industrial economic institution, and those which are inextricably tied up with it.

This discussion is pertinent today when many non-industrial societies are anxious to industrialize. A growing industrial plant is seen as security from economic want, economic imperialism, and military threats. In the short run national pride may encourage deliberate policies to perpetuate old customs and beliefs side by side with the new importations. Some of these short range compromises with the old social system may run against the long range interests of the national leaders.

Knowing what must accompany industrialization would save time, money, and effort, and prevent confusion. Any attempt by planners to stop an inevitable social

change accompanying industrialization at the same time that industrialization is being encouraged, will increase disorganization and make it impossible for them to achieve their goals. On the other hand, knowledge of what need not change may be used to soften the impact of industrialization, appease vested interests to some extent, and intelligently blend the new with the old. This paper will contribute only indirectly to the solution of the latter problem.

The first half of the discussion will deal with the disorganizing effects of the change in social relationships engendered by the participation of members of a non-industrial society in activity oriented to modern machines. The second half of the paper will deal with the new patterns of social relationships which will develop in relation to the demands imposed by the machine.

### DISINTEGRATION OF CERTAIN ASPECTS OF THE OLD ORDER

"Every society is an organized entity. . . . New technological practices are disruptive to such entities. . . ." <sup>2</sup> Thus social disorganization is a short range result of the decline of the old system.

One important change which will occur in a society with the introduction of industrialization will be the creation of new

<sup>1</sup> The special case of the mechanization of agriculture will not be discussed here.

<sup>2</sup> Walter R. Goldschmidt, "The Interrelations Between Cultural Factors and the Acquisition of New Technical Skills," in Bert F. Hoselitz (Editor), *The Progress of Underdeveloped Areas*, Chicago: The University of Chicago Press, 1952, p. 139.



roles. These new roles are predominantly economic in nature—thus introducing an entirely new pattern of behavior into a society. Previously there had been no predominantly economic roles. Economic behavior was performed in roles which integrally combined other institutions with the economic. In non-industrialized and especially non-literate societies economic "... forms which we differentiate quite sharply are not only indistinguishable, but in many instances are so intimately linked with the non-economic institutions that we can only discern them at all by giving the closest attention to their economic role."<sup>3</sup>

This separation of the economic functions from a totally integrated system, would throw the entire system into a state of disequilibrium. All role behavior in an integrated system of human interaction tends to maintain and support other aspects of the system. The indigenous power structure is weakened through its loss of control over the economic sanctions. "Schapera describes the influence of money economy upon the family system of the Kgatla, a Bantu people of Bechuanaland. Here . . . the marriage was arranged by the elders. But with urban employment the sons were released from economic dependence upon the fathers, they were given physical separation from the cultural system, and their marriages were perforce delayed. All three factors combined to make the youth act independently in seeking a marriage partner, to undermine the authority of the parental generation, and to lessen materially the unity of the family as system of interdependence."<sup>4</sup>

Another factor which further disrupts the old system is the decline of certain old roles, such as skilled craftsmen and magicians. This development would incur the opposition of those individuals whose positions are challenged. This was true in the case of the Kgatla cited above.<sup>5</sup>

It may be hypothesized that the conservatism encountered in the form of opposition to industrialization is to a considerable degree resistance on the part of those who expect to lose most from the

changes, and not alone due to the resistance of deep seated values. This interpretation may be implied from Wilbert E. Moore's discussion of the problems involved in recruiting indigenous labor for the factory.<sup>6</sup> The fears of the threatened members of the non-industrialized society are based on sound fact, and are not simply an irrational reaction to change. Not only are their skills declining in importance, but new facilities have been created which give their possessors new sources of instrumental power. The people who have gained the new facilities (monetary gains for the factory workers)<sup>7</sup> become economically independent of the old order.

The creation of new facilities leads to the development of new rewards. The facilities themselves as signs of instrumental power become rewards to some extent.<sup>8</sup> For example, in the United States money, which is in reality a facility for instrumental activity, has come to be regarded as a reward and a source of direct gratification. In the newly industrializing society the new facilities of the factory workers consist primarily of skills, tools, and money. These facilities, having become rewards, place prestige and higher social status in the hands of new individuals in the society. It is not that these factory workers are supplanting the old elite, but they are rising above agricultural and craftsmen classes to which they were previously inferior, assuming that the poorest and lowliest people are the ones first attracted to the factory.<sup>9</sup> An individual making a choice will always seek to optimize his gratifications. Apparently the persons receiving the least gratifications under the old system will be the first to be attracted to the new possibilities.

Thus far we have discussed those facilities which have been introduced into the society for the first time by industrialization. However, there are elements in the old social system which had not been important

<sup>6</sup> Wilbert E. Moore, *Industrialization and Labor*, Ithaca: Cornell University Press, 1951, Chapter 2.

<sup>7</sup> It is assumed that there is no question of the fact that the use of money accompanies industrialization.

<sup>8</sup> Talcott Parsons, *The Social System*, Glencoe, Illinois: The Free Press, 1951, p. 513.

<sup>9</sup> Moore, *op. cit.*, p. 304.

<sup>3</sup> Melville J. Herskovits, *Economic Anthropology*, New York: Alfred A. Knopf, 1952, p. 155.

<sup>4</sup> Goldschmidt, *op. cit.*, pp. 140-141.

<sup>5</sup> *Ibid.*, p. 141.

<sup>10</sup> R. tors A. *op. cit.*

previously, such as certain skills and resources, which now become significant. These skills and resources become more important as new symbols of prestige, at the same time that older symbols in the society are losing their former prominence. Thus certain aspects of the old system are discouraged by the introduction of the industrial system, while other aspects become more important. The industrial system tends to respect people who work with their hands, while "... in all the unmechanized civilizations the trader and the mechanic rank far down in the social scale."<sup>10</sup> The industrial system values even more highly certain other skills such as executive and scientific abilities, but these mental skills are quite different from those possessed by the old elite.

The creation of new roles, new facilities, and new rewards, and the prominence of previously unimportant resources and skills, all tend to place new individuals in positions of power. The new power structure challenges the old, and later tends to produce a new elite. Even the common factory workers may come to have more power and influence than certain previously higher classes who remained in agricultural pursuits. This gain of power and influence is even more pronounced in the case of the entrepreneurs, especially when they rise from within the society, and are not merely foreign investors and managers.

Even if the new elite merely consists of members of the old elite this does not affect the analysis. The fact that they are the same individuals makes no difference, for they will have to have new attitudes, behavior, expectations, and values, in order to perform their new roles successfully. Thus from the point of view of the social system there is an entirely new elite. On the other hand, if the new elite consists of individuals who have risen from below and who seek to incorporate themselves into the old elite, they necessarily will modify the old elite in the process. They must maintain role behavior and orientations, compatible with their positions in the industrial system, which will not be the

same as the traditional orientations and expectations of the old elite. In either case the analysis holds and the power structure of the society is modified.

A further development which is disruptive of the old order stems from the fact that industrialization always leads to the production of large quantities of cheap goods. Even in a country where a low wage policy prevails, the people will have enough money to buy at least some of the products of industrialization. The mass production of large quantities of cheap goods will make available to them material products which they never before dreamed of possessing. This will be true not only of the factory workers but also of those who have remained in agriculture, for the growing cities will provide an opportunity to sell surplus produce at a profit. This is an incentive to produce more, and the new agricultural techniques and implements make possible the additional production. This will extend to every village within the circle of the new industrialization. Thus some degree of improvement of the common people's standard of living will occur. However, in countries where a small group wishes, and is able, to keep most of the benefits of industrialization to itself, this improvement will not be very great. Low wages, systems of land ownership, and systems of taxation may keep the majority of the people poor. This situation of only slight improvement for the majority cannot last forever because industrialization eventually provides a greatly improved standard of living for everyone. It may, however, last for some time, long enough to create an explosive situation in the short run.

When people have for centuries held a fatalistic attitude, bad conditions are accepted, and this helps to maintain the stability of the social system. "In a society in which people regularly expect to be hungry annually, and in which traditions and proverbs accustom them to expect such a period of privation, their whole attitude toward economic effort is affected. . . . Among the Bamba scarcity is within the ordinary run of experience, and accepted as such."<sup>11</sup> Some degree of amelioration may

<sup>10</sup> Ralph Linton, "Cultural and Personality Factors Affecting Economic Growth," in Hoselitz, *op. cit.*, p. 85.

<sup>11</sup> Herskovits, *op. cit.*, pp. 293-294, quoted from Audrey J. Richards.

open their thinking to the possibilities of even greater improvements, and, ironically, lead to impatience and general dissatisfaction. Thus the people of the areas we are trying to protect from Communism may become more receptive to the seemingly quick and easy solutions offered by the Communists. There should be a sensitivity to the potential dangers of a slow process of aid to "underdeveloped areas" by those in charge of these programs.

In the section above the causes of the disintegration of the old order have been discussed. There are, however, certain forces tending to delay this disintegration, thus prolonging the period of disorganization. Among the most important of these is the continued emotional dependence of the workers on the old community. This emotional dependence will cause them to conform to some degree to the normative patterns of the community. This brings them under the influence of the embattled conservative leadership. Thus the emotional dependence provokes modes of orientation which slow down the adjustment of the native factory worker to the new industrial element in his environment. It must be emphasized that the advent of industrialization introduces only instrumental activities, and does not constitute a total functioning social system. When the native recruit is unhappy and discontented in the new factory situation it is not simply that he is nostalgic for the traditional atmosphere of his youth. Although there is this longing for the old way of life, a much more important and more elusive point is that man gains most of his satisfactions and feelings of security from a well rounded and integrated pattern of interaction. This means that he needs an orderly life based on mutual patterns of expectations that cover all phases of his activities within and without the immediate factory situation. The old patterns of interaction and patterns of expectations are inadequate in a system which involves an entirely different economic orientation. Thus social relationships are disrupted. There is a great deal of insecurity, and the morale and efficiency of the factory worker are greatly reduced.

A new social system is needed in the industrial community, a system which would integrate the new economic system with

those aspects of the old culture which can be adjusted to industrialization. It should provide reasonably integrated patterns of action and expectations covering all interaction among individuals, no matter how close or how distant. Where the native population has no such integrated system of expectations, the period of adjustment will be longer than where one exists.

An interesting problem is posed here for those who wish to increase the efficiency of native labor, hasten their adjustment to the non-village life in the growing factory areas, and to increase the number of workers who come and stay at their new factory positions. The above analysis would imply that some ambitious planner should try to encourage the development of new patterns of expectations in the various institutions of the newly developing social system. This would demand first of all some knowledge of the form the institutions will have to take so that no patterns of expectations will be introduced or encouraged that would later interfere with the projected goals of industrialization envisaged by the national leaders of the "underdeveloped" countries. It is not within the scope of this paper to suggest methods of hastening the development of new patterns of expectations, but the general direction of this development will be discussed in the following section.

So far, the discussion has been concerned with the decline of the old order through the creation of new roles, new facilities, new rewards, and new sources of prestige and power. The resultant general picture of social disorganization presented above is not likely to remain permanently. There is a tendency to restore a relative equilibrium in any ongoing process of interaction.<sup>12</sup> Further changes will develop which will provide the basis for this new integration.

#### REORGANIZATION OF THE SOCIAL SYSTEM

The reorganization of the society can be analyzed in terms of four of Talcott Parsons' five pattern variables.<sup>13</sup> The thesis

<sup>12</sup> Parsons, *op. cit.*, pp. 481-482.

<sup>13</sup> Briefly the four pattern variables may be defined as follows: (1) *Specificity*—concern with only one aspect of a person; *diffuseness*—concern with the total person. (2) *Particularism*—orientation to a person on the basis of some special relationship to him based on membership in a certain

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advanced here is that an increase in universalism, achievement, suppression of immediate emotional release (affective-neutrality), and specificity all accompany industrialization in the long run. The remainder of this paper will be concerned with the dynamics of this development.

Industrialization means the introduction of machinery, and many leaders of non-industrial societies have expressed the view that machines, and *only* machines are what they want from the industrial world. This is certainly an oversimplification of what industrialization means. Because machines are very expensive they necessarily must be used economically. In the early stages of industrialization labor is far less scarce than machinery. This means that there is a strong constraint on the part of those related to the industrial process to adjust labor to the machines. It is very difficult to tolerate inefficient use of the machines, insofar as this can be avoided. The necessity of teaching and enforcing these modes of adjustment demands a certain type of social organization. This social organization centered about the need to adjust to the machines in the factory system engenders certain unique social relationships. The native's first major change in the patterning of his relations to others is experienced in the factory situation.

#### THE NECESSITY TO ADJUST TO MACHINERY AND ITS EFFECTS ON SOCIAL ORGANIZATION<sup>14</sup>

The first adjustment a man has to make to machines is in the long hours he has to

collectivity (e.g. a kinship grouping); *universalism*—orientation based on the possession of a certain attribute or attributes (achieved or ascribed) regardless of the particular person who possesses it. (3) *Achievement*—emphasis on actual performances of an individual; *ascription*—emphasis on certain qualities of an individual, either given at birth or automatically conferred later, regardless of performances. (4) *Affectivity*—giving open expression to immediate desires either to do or not to do something (immediate emotional release); *affective-neutrality*—suppressing immediate desires for a long range interest (suppression of emotional release).

The pattern variable of self-orientation vs. collectivity-orientation is not included in this paper. For a fuller discussion of the pattern variables see: Parsons, *op. cit.*, pp. 58-67.

<sup>14</sup>In terms of M. J. Levy Jr.'s conceptual scheme as developed in *The Structure of Society*, this is an analysis of the functional requisites of

spend away from his home community. The extent to which he is away from his old ties determines the degree to which his relationship toward them will change. A long period of the day goes by in a new social situation without the supervision of the old normative system. Furthermore, his relationship to his family changes from the old pattern of responsibilities and expectations which depended on his being with them a good deal of the day. Spending less time in his home community also means that the close relationships formerly possible cannot be the same. Since the immediate family is in the end the most intimate and most psychologically satisfying human group, the worker, with less time, will choose to spend proportionately more of that time with his immediate family, and less time with his extended family<sup>15</sup> and the others in the community. Devoting more of his waking hours to machines, then, means less time with his family, less time with his home community, and proportionately more time with his close family than with his neighbors. Because his constant close relations with his neighbors decrease substantially, his relations to them change. His relations are less diffuse, because he has less time to know them thoroughly. They are less particularistic because there are fewer groups in which he and his neighbors are integrated. Because he is dealing with relatively unfamiliar people on a specific-universalistic basis, his relations with them will be less emotionally involved.

Secondly, because the machines are expensive and complicated the individuals working with them must be selected on the basis of achievement.<sup>16</sup> Thus, in the

the industrial system and the structural requisites (particularly of relationship structures) that logically and necessarily follow therefrom. This paper will particularly emphasize the derivation of the structural requisites from the functional requisites. Marion J. Levy, Jr., *The Structure of Society*, Princeton: Princeton University Press, 1952.

<sup>15</sup>Some writers have noted the disintegrating effect of industrialization on the extended family. Ralph Linton comments, "Modernization of the unmechanized culture, . . . cannot fail to weaken or even destroy joint family patterns." From Linton, *op. cit.*, p. 84. Talcott Parsons expresses the same view on pages 178 and 510 of *The Social System*.

<sup>16</sup>"Advanced degrees of industrialization greatly accentuate the importance of differences in ability

factory situation achievement becomes dominant over ascription. The ability to run the machines is a specific demand, and diffuse standards become irrelevant. Thus the pattern variable of specificity is further strengthened. Furthermore, it is difficult to combine particularism and achievement as a general pattern. A person's cousin may not be as qualified as someone unrelated to him. It is impossible in the long run for the extended family to comprise the factory unit because the criteria for the old status system of the family will not fit the criteria of the factory (physical strength, dexterity, and the like). Achievement-universalism will be fostered not only by the demand for efficient labor for the expensive machines, but also by the demands imposed on the factory unit by competition from domestic or foreign competitors. The Japanese attempt to integrate the new industrial system with the old familial organization shows unmistakable signs of disintegration.<sup>17</sup>

Third, while in the non-industrialized societies discipline is self-imposed, that of the industrial operative is imposed from outside. This routine is imposed on the worker because he is no longer self-employed, because the great expense of the machinery and fuel demands constant operation, and because of the demands for coordination imposed by the greater division of labor. One of the most serious problems facing the new factory in the underdeveloped area is that of absenteeism and labor turnover due to the reluctance of the workers to accept the new factory discipline.<sup>18</sup> The worker must do his work regardless of his private desires of the moment. Gratification must be postponed.

This continuous attention demanded by the machine and the restrictions thus im-

posed on the worker, in turn, lead to a new kind of relationship between him and his superiors, and between him and his fellow workers. Before the introduction of the machine the relation of the individual member of the working group to those who possessed authority was diffuse. In the factory situation the relation of the individual toward his superiors becomes very specific. The superiors are only interested in those aspects of the individual which are relevant to the efficient use of the machines. This is one of the important changes in the relations of the worker which initiates the process of separating the economic from the other institutions in the social system.

In the factory situation the relations of the individual worker to his equals can no longer be based on the diffuse relations which existed when each member of a working group knew every other member as a total individual because of constant contact. The laborers in the factory spend a good deal of their time there, and the main contact they have with the majority of their fellow workers is in this context. All they know of the majority of their fellow workers is their achievements, skills, and personalities as production workers. Evaluations and adjustments are in terms of the factory system. Thus specificity is necessarily dominant. In Japan the attempt to maintain diffuse relationships in the factory system was possible only in the short run.<sup>19</sup> Strains were produced in the Japanese attempt to adapt the machine to their premachine society. These strains eventually led to the breakdown of the system of diffuse factory relations, and to an adjustment more consistent with a machine society which demands the separation of the economic from the other institutions.

Since the economic system has become separated from the other systems, economic considerations become of prime importance in judging individuals and allocating roles within the economic system. With further specialization of the industrial system only one particular skill becomes relevant. Thus, in selecting an electrician, only that skill is significant, and literary and artistic talents, religious and political beliefs, and

<sup>17</sup> Moore, *op. cit.*, pp. 30-31.

<sup>18</sup> Moore, *op. cit.*, Chapter 5.

<sup>19</sup> Moore, *op. cit.*, pp. 30-31.

sociability are not considered. Since instrumental activity is the only kind of performance demanded by these roles, diffuse and particularistic considerations are of no relevance to the individual's ability to perform the role satisfactorily. Although this specificity is not always complete in all situations in an industrial economy, still it is the dominant pattern and the accepted value.

A fourth effect of industrialization is a further growth of specificity due to a new orientation to a larger community. Mass production produces a huge quantity of goods which must be sold to a large and far flung number of people. It would be impossible to know all these people thoroughly—which would be necessary if diffuse standards were to apply. The modern market by its nature demands specific orientations. At the same time the scope of political power grows with industrialization and the authorities of a large nation cannot possibly deal with all citizens on the basis of diffuse standards. In addition, the vastness of the new economic and political relationships are as disruptive of particularism as they are of diffuseness. Time considerations alone demand a universalistic orientation in the large modern market or nation.

Another impetus to the specificity-universalistic complex is the impersonality of the modern factory. This impersonality is due to the large size of the modern productive enterprise and also to the need for large amounts of capital, which in turn leads to the corporate system with its absentee stockholder owners. In the factory the managers and workers are for the most part strangers to each other, and this is certainly true of the owners and workers, who often never meet. The only kind of standards which can be used for allocation of roles in such a situation must be specific and universalistic.

Since the economic system is very important and generally the dominant system in an industrial society, its values tend to dominate the other systems. Levy writes, "... it is impossible to confine these patterns rigidly to the economic aspect of action. . . . Highly universalistic relations in the economic aspects of action are functionally incompatible with highly particu-

laristic ones in the political (i.e. allocation of power and responsibility) aspects of action. It is because of such functional interrelationships among different spheres and aspects of action that these clusters of patterns seem on the whole to appear widely diffused in social systems rather than narrowly confined."<sup>20</sup>

The kinship system is an exception to the above analysis for by its nature it is diffuse, emotional, particularistic, and predominantly ascriptive.<sup>21</sup>

#### CONCLUSION

There may well be strong attempts on the part of the leaders of some of the industrializing countries to integrate some of the old patterns of human relationships into the newly developing industrial society. It is the opinion of the writer that some of the leaders of these non-industrial societies are in a sense marginal men, or men of two cultural systems. They are emotionally tied to the old system, but at the same time they are anxious to reap the benefits of a system which promises to relieve some of their economic problems. This conscious attempt on the part of many non-industrialized countries to integrate the old culture with a complex technology quite foreign to them can be successful only insofar as the old cultural elements do not, in the long run, conflict with certain patterns of human relationships basic to a machine society. This integration could provide cultural continuity which would most certainly ease the transitional period, make it more productive, and hasten the process of industrialization. However, insofar as old cultural elements are preserved in the face of the demands of the new and promising factory system, there will be additional strains that will have to be overcome in the long run, such as less productivity, and a serious delay in the achievement of the desired basic economic goals. Another interesting question, not within the province of this paper, is the question of what kinds of intermediate adjustments could be made which would tend to support old social relationships, but in which provision is made for their slow

<sup>20</sup> Levy, *op. cit.*, pp. 122-123.

<sup>21</sup> Parsons, *op. cit.*, p. 186.



dissolution. This writer thinks, however, that this approach would be unrealistic, despite its short run advantages.

While this paper has discussed some of the long run consequences of industrialization for underdeveloped areas, it does not imply that there cannot be any cultural continuity, or cultural forms which are peculiar to the society in question. A good deal of variation in industrialized societies is possible. There is no evidence, for exam-

ple, that any of the following have to change: music, art, religious beliefs about the non-empirical world, and many folkways.

It must be reiterated that there is no implication that any non-industrial society must accept the pattern of development outlined in this paper. However, insofar as a society does accept the value of industrialization and seeks to bring it about, then the development discussed above is to be expected.

## THE EURASIAN MINORITY IN INDONESIA

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**P**ERHAPS no single population group among the races and peoples of Indonesia has aroused so much interest since the war as the Eurasian minority. Both in the Netherlands and in the Indonesian Republic the problem of the Indo-European or Indo, as the Eurasian is generally called, has reached a certain acuteness, which contributes to the steadily deteriorating relationship between the two countries. It is the purpose of this paper to examine the origin of the Indo problem, its status in colonial and in present day Indonesian society, and to suggest possible means of resolving it.

The Indo problem is aggravated by the fact that there are no precise data on the number of Eurasians. The last time that an effort was made to count the number of Eurasians in Indonesia was in 1854, when it was found that over 9,000 of the 18,000 Europeans in Java possessed what was referred to as "the characteristic skin color" of the Indo, while an additional 5,600 of Europeans born in the Indies without this telltale mark were also regarded as being of mixed blood. Colonial law classified the Eurasian as European, insofar as he had legally been recognized as such, but it is likely that the number of Eurasians far exceeds even the 300,000 persons classified as Europeans in 1941. For in this 300,000 are not included the thousands upon thousands of mestizos with full blood European ancestors who through circumstances

were absorbed by indigenous Indonesian society and thus came to be classified legally as "native." According to one observer, the total number of mestizos with white ancestors among the Indonesians in 1940 was from 8 to 9 million.<sup>1</sup> This figure has been criticized as too large, but even so it should be noted that the overwhelming majority of persons classified as Europeans by colonial law were Indos; according to one estimate 90 per cent of the legally categorized Europeans were of mixed blood around 1930,<sup>2</sup> and while the full blood European group increased in the decade 1930-1940, this percentage is probably still roughly accurate for the composition of the European group at the outbreak of the Second World War. Since the Japanese occupation and the Indonesian Revolution the Indo group has generally declined; hundreds perished during the occupation and its aftermath, tens of thousands have migrated to the Netherlands and to Western Irian (New Guinea); in Indonesia some retain their exclusive "Indo" orientation while others have "assimilated" with the Indonesians as is expected of good *warganegara* (citizens) of a national Indonesian

<sup>1</sup> Data from A. van Marle, "De groep der Europeanen in Nederlandsch-Indië, iets over ontstaan en groei," *Indonesië*, V (1951), p. 106; V (1952), pp. 487-488; W. F. Wertheim, *Het sociologisch karakter van de Indo maatschappij*, Amsterdam, 1947, p. 5.

<sup>2</sup> J. Th. Koks, *De Indo*, Amsterdam, 1931, p. 19.

state and have attempted to de-emphasize their "Indo" status with varying degrees of success.

Even before the coming of the Dutch there had been Portuguese mestizos in the Indies. Later when the Dutch secured a foothold in the archipelago some of these mestizos came under the control of the Dutch East India Company. The city of Batavia, the chief Dutch settlement and the headquarters of the Company, soon contained a large number of Eurasians, some the descendants of Portuguese mestizos, others of *Mardijkers* and *Papangers*. *Mardijkers* (from the Portuguese *mahardika* or "free man") had originally been Christian slaves in Portuguese service who were captured by the Dutch; most came from Bengal and India. *Papangers* were Philipinos, originally brought by the Spaniards to the East Indies and taken prisoner by the Dutch. Both groups served the Dutch as mercenaries and since they were Christians their treatment was far better than that accorded native Indonesians. *Mardijkers* and *Papangers* in the course of time intermarried with each other and with Portuguese mestizos. The result was that the first Eurasian groups in the Dutch colonial society were a distinctly alien cultural element: their names, language, customs and manners were a mixture of Malayan and Portuguese and as "Portuguese" they were known and continued to exist as a distinct Indo minority until deep into the nineteenth century.<sup>3</sup>

Apart from the "Portuguese" group—though later mixed with it—existed a growing group of so-called *Mixtiezen* (mestizos), usually the offspring of illegal unions. Depending on the degree of purity of their European blood they were categorized as *Castiezen*, *Pustiezen* and *Christiezen* (roughly one half, one quarter, and one eighth Indonesian respectively), terms which had no legal importance but did indicate differences in social standing.<sup>4</sup> For the attitude of the full blood Calvinistic Netherlander toward "Portuguese" and mestizos was

generally colored by a resentment of their "foreign" manners and their religious practices and by a sense of racial superiority. Although in the days of the Company the population was divided on the basis of religion and not of race<sup>5</sup> and the Eurasian had a status theoretically on a par with the full blood Christian, in reality discrimination was quite apparent socially as well as in employment practices.<sup>6</sup> As early as 1617 whites who had married with a native Indonesian were denied repatriation, later this prohibition was extended to those married to a Eurasian.<sup>7</sup> Mixed marriages themselves were not prohibited, provided both partners were Christians. Due to the acute shortage of European immigrant women such unions flourished through the centuries. Yet Dutch writers in Company days continued to denounce Eurasian women as lazy, immoral and generally worthless,<sup>8</sup> though the European women occasionally imported were certainly not of better quality. Indonesian women—later especially imported from Bali—swelled the settlement of Batavia too. Their generally illegitimate Eurasian offspring occasionally succeeded in maintaining a sort of "honorary" status, but most led a peculiarly marginal existence on the brink of poverty and disaster.

In the nineteenth century the Eurasian problem comes more forcibly to the fore. The end of the Company and the advent of national control over the Indies made the Eurasian a Netherlands subject, yet different legal classifications prevented any public homogeneity from coming into existence. The prized "European" status was no longer given to most Christians, descent from a European father or legal recognition by him

<sup>5</sup> W. E. van Mastenbroek, *De historische ontwikkeling van de staatsrechtelijke indeeling der bevolking van Nederlandsch-Indië* (Dissertation; Wageningen, 1934), p. 46.

<sup>6</sup> In 1676 the Company was ordered to employ as few mestizos as possible, earlier they were barred from holding positions as clerks. In 1753 Company officials born in the Indies were deprived of pension. Koks, p. 27.

<sup>7</sup> Koks, p. 13.

<sup>8</sup> See for example the classic account of Nicolaus de Graaff, published in 1701 in E. du Perron, ed., *De Muze van Jan Companje*, Bandoeng, 1948, pp. 111–117.

<sup>3</sup> F. de Haan, *Oud Batavia*, 2nd ed., Bandoeng, 1935; D. de Iongh, *Het Krijgswezen onder de V.O.C.*, The Hague, 1950, pp. 62–66; H. de Graaf, *Geschiedenis van Indonesië*, The Hague, 1949, pp. 173–174.

<sup>4</sup> De Haan, p. 420.

now had to be proved.<sup>9</sup> Many Eurasians "went Indonesian," others—in the absence of any government care—eked out an increasingly difficult existence as clerks or petty officials. Their ranks continued to grow, however, thanks to the tacit recognition given to two hallowed Dutch colonial institutions: barracks concubinage and the planter's *njai* (housekeeper). Concubinage has had a lengthy tradition in Dutch colonial society. In the later days of the East India Company there were more illegitimate births out of Christian fathers than legitimate ones, while in the middle of the nineteenth century concubinage was an accepted part of the mores of the city of Batavia and the illegitimate spouse received the same respect as the legitimate one. Among the European military of lower ranks in the Indies concubinage had been permitted since 1836. In 1888, for example, more than 22 per cent of European military lived with their concubines in the barracks, in 1909 almost 26 per cent; later the system declined as European women became more plentiful. But in 1902 the number of illegitimate children of European military born out of concubinage was still twice as large as the number of legitimate ones. Not all of these children were "recognized" by their fathers, many disappeared into Indonesian society; sometimes professional "recognizers"—retired European military who for a fee of a couple of bottles of gin were willing to declare anyone as their offspring—did the job, and thus the prized "European" status was attained. In the group of officers, officials, planters and professional men from the highest to the lowest conditions were not much better. The shortage of European women and the fear of jeopardizing one's future by a *mésalliance* with an Indonesian led to the widespread practice of keeping *njais*: of the Preangar region in Western Java south of Batavia, one government report noted in 1902, that of the hundred unmarried males over eighteen, ninety were living in concubinage.<sup>10</sup>

Government provisions for integrating this growing class of Eurasians as useful members of colonial society, were woefully

inadequate throughout the nineteenth century. Educational facilities were few and of inferior quality, few Eurasians received any training in Europe, and so they had to be content with the lower positions in government and private enterprise. Though life in the European community in the nineteenth century was peculiarly adapted to its Indonesian surroundings and a certain colonial Eurasian culture pattern predominated, the Eurasian himself was often a second class member of this community, an object of ridicule to Dutch immigrants and to Dutch colonial novelists<sup>11</sup> alike, who sneered at his broken Dutch, his aping of European manners, and his pretensions to social equality. Neglect often forced the Indo into the ranks of the paupers, living on the edge of the Indonesian *kampung* (village or native quarter) or ultimately disappearing into it. For this group of the population a white skin color, perfect "Dutch" manners and European legal status became major, deeply desired and yet remote goals in their existence. The author of a useful novel about the poverty stricken Indos, described these ambitions well: the Indo girl "will surrender everything in order to be married to a white man; and the unexpressed thought is: the desire for white-skinned children."<sup>12</sup>

In the nineteenth century the Indo community began to display its inner social structure more clearly. At the top were the few who had attained a measure of wealth and respectability as estate owners and entrepreneurs. In this group also belong the members of two government branches, which for a majority were composed of Indos, namely the commissioned ranks of the army and the civil service. But in terms of the total Indo community this group was a minority, furthermore Dutch import-export concerns and Western enterprises generally discriminated against Eurasians. The small Indo élite retained its position solely by virtue of its superior education and training obtained in Europe, even so one authority has described their existence as "a continuous struggle for equal rights in government

<sup>9</sup> W. F. Wertheim, *Herrijzend Azië. Opstellen over de Oosterse samenleving*, Arnhem, 1950, p. 63.

<sup>10</sup> Data from van Marle, pp. 481-486.

<sup>11</sup> Cf. G. Brom, *Java in onze kunst*, Rotterdam, 1931, pp. 90-91.

<sup>12</sup> Victor Ido, *De Paupers*, Batavia, 1930, pp. 65, 101.

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positions" with those born in Holland.<sup>13</sup> Generally their style of living was that of landed gentry; their houses were large and spacious, with an abundance of servants, they "breathed an atmosphere of hospitality and generosity." Toward the Indonesian the Indo élite had a paternalistic attitude, a sense of the "grand seigneur."<sup>14</sup> By the twentieth century this largesse and semi-aristocratic opulence had largely declined however, nor was this class even in the nineteenth century free from the prejudices of the full blood Dutch group.

The second class and middle layer of Indo society was composed of a large number of petty officials, clerks and subalterns with some form of education beyond the elementary level, whose life in most cases was European in name only. For most of them the struggle to retain their position was even more difficult than for the first class, although during most of the nineteenth century their modest jobs were fairly secure. Something of the semi-aristocratic style of living also penetrated to them (hospitality, love of hunting), but in this group the consciousness of the prized European status was far stronger and the efforts to retain it and act in accordance with it far more contrived. The old "Portugese" group belonged largely to this second class although by 1900 their earlier "Portugese" characteristics had been lost. The lowest rung of the social ladder in Indo society was occupied by the pauper element, living on the border of or in the *kampung*, leading an existence largely Indonesianized and precariously balanced on handicraft, trade, or more often on organized crime. Most of the paupers had had a smattering of education and had a passing acquaintance with the Dutch language in some form. In these last two groups the enmity toward the growing group of full blood European immigrants (*totoks*) was perhaps most intense and the peculiar minority myth of social superiority to both Indonesians and *totoks* was perhaps strongest.<sup>15</sup>

Due to the lack of social incentive and of government care, in the absence of proper educational facilities, the majority of Indos were assigned a social status in which they

were forever made to feel inferior and a target of ridicule. Their inability to speak Dutch fluently was criticized, yet the facilities to teach them to speak the language properly were altogether inadequate; their indolence and supposedly inferior intelligence was said to be "proverbial," yet incentives to emancipation were withheld. In the twentieth century these aspects of the Indo problem became even more acute. The growing number of European immigrants constituted an increasing source of competition in the more rewarding positions in the administration and in private enterprise. The colonial culture pattern of the previous century disappeared, European society became more "European,"<sup>16</sup> more oriented toward the West and even less inclined to accept the Indo as one of its own. At the same time the Indo petty official class began to experience the competitive pressures of trained Indonesians and its position became extremely precarious. With the development of political life in the first two decades of the present century the Indos joined hands in the establishment in 1919 of a party of their own, the *Indo Europeesch Verbond* (IEV) with a program that stressed economic assistance to and social emancipation of the Eurasian. Politically IEV was not very successful, even though its delegation in the *Volksraad*, colonial Indonesia's semi-parliament, was consistently one of the largest.<sup>17</sup> For its spokesmen were in the habit of advocating the unity of all Netherlanders born in Indonesia, including those with full blood parents and of classifying as Indos all Dutchmen whose "lasting interest" lay in the Indies. At an earlier time this advocacy of a common interest between full blood and Indo might have found support; in the twentieth century however, with its steady "Europeanization" of the full blood community and its increased class conscious-

<sup>13</sup> J. S. Furnivall, *Netherlands India. A study of plural economy*, New York, 1944, pp. 405-406, 418.

<sup>17</sup> See representation tables in *Verslag van de Commissie tot bestudeering van staatsrechtelijke hervormingen, ingesteld bij gouvernementsbesluit van 14 September 1940*, No. IX/KAB, Batavia, 1941, Vol. I, pp. 81-93. On the IEV see also A. C. van den Bijllaardt, *Ontstaan en Ontwikkeling der staatkundige partijen in Nederlandsch-Indië*, Batavia, 1933.

<sup>13</sup> Koks, p. 263.

<sup>14</sup> Wertheim, *Herrijzend Azië*, p. 64.

<sup>15</sup> Koks, pp. 228-254.

ness, such an appeal largely fell on deaf ears. Many full blood Europeans born in the Indies who in their style of living might have approximated the better educated Indo gradually turned from any assimilation with the mestizo, and their children increased the distance between themselves and the Eurasian even further. In the words of one student of the problem:<sup>18</sup>

In earlier years children born in the Indies conceived of themselves as "Indian children," regardless of their origin, they talked with the same accent, had the same customs and laughed together at the few, Holland-born 'cheese-heads' in school. But in later years something of the social antithesis between whites and Eurasians also had penetrated into the world of children.

The lines of class demarcation became sharper in the period of the IEV's existence; as a result its appeal to concord with Indonesia-born full bloods was criticized as an attempt to climb to an élite status in society on the back of the full blood. Notwithstanding social rebuffs the IEV generally persisted in this orientation and in the course of time developed a marked, pro-Dutch conservatism, an attitude of *plus Hollandais que les Hollandais*, that led not a few of its members to espouse the cause of the NSB, the colonial fascist party in sympathy with Hitler, whose racial ideologies they apparently felt were not inconsistent with their own origin and status. Some IEV leaders even went so far as to announce the ultimate creation of a Eurasian society in Indonesia, a social nucleus around which both Europeans and the Indonesian intelligentsia would be integrated and which would peaceably resolve the colonial conflict between Hollander and emancipated Indonesian. Hence the only viewpoint which would in the end exist in Indonesia according to this opinion would be the "Eurasian" one.<sup>19</sup> Pronouncements such as these not only antagonized the full blood Dutch element, but what was perhaps worse also the leaders of the growing Indonesian nationalist movement to whom the IEV had long since become anathema.

The realization that he really could not

do without a continuation of Dutch control, even though on occasion he too demanded greater autonomy for Indonesia within a Dutch Commonwealth, was primarily responsible for the Indo's conservative bent in the decades just before the war. His actions continued to reflect his paradoxical position in colonial society. On the one hand he insisted that he was a "European" and expected to be treated as such, yet it was as an Indo that he informed the government of his economic plight and asked its assistance. It was as a "European" that he joined the ranks of colonial die-hards, but as an Indo complained that he could not make a living and asked the government to consider the sale of land to him, something forbidden to all non-native Indonesians by colonial law.<sup>20</sup> It was as a European that he informed a government committee investigating political wishes in 1941 of the discrimination against him by private Western enterprises,<sup>21</sup> but it was as an Indo that he urged the government to make of Western New Guinea a colonization area primarily for Eurasians. Yet at one time, early in the twentieth century, there had been a good chance that the Indo might find a home in a group or party that advocated the political and cultural assimilation of all population groups, Europeans, Eurasians, Indonesians, and other Orientals, and that his precarious position would resolve itself into ethnic and social harmony. Such a party, the Indische Partij, had in fact been founded in 1912 by a talented Eurasian, E. Douwes Dekker, who had hoped to make of it a forum of progressive political action, regardless of racial origin.<sup>22</sup> The caste consciousness and social stratification of the period between the two world wars wrecked such assimilation schemes, although a few Eurasians, like Dekker, continued to propagate the idea that the future of the Indo lay in union and assimilation with the Indonesian, not in the retention of Dutch imposed class consciousness.

Except for this minority of Eurasians, relations between the Indos and the Indone-

<sup>18</sup> Wertheim, *Herrijzend Azië*, p. 69.

<sup>19</sup> Cf. the address of V. Ploegman in W. van Helsdingen, *De Plaats van Nederlandsch-Indië in het Koninkrijk*, Leyden, 1946, Vol. I, pp. 61-63.

<sup>20</sup> See *Verslag van de Commissie voor het Grondbezit van Indo-Europeanen*, Batavia, 1935.

<sup>21</sup> *Verslag van de Commissie tot bestudeering van Staatsrechtelijke Hervormingen*, Vol. 11, p. 87.

<sup>22</sup> Van den Bijlaardt, pp. 6-18.

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sians deteriorated steadily. The growing class of educated native commoners with their nationalist aspirations competed increasingly with the Indo in all walks of life traditionally regarded as "Eurasian." Whatever the Indo did to defend himself aroused the antagonism of the Indonesian: if he pointed to his loyalty to the Dutch regime, he gave the impression that the Indonesian was disloyal; if he pointed to his long period of faithful service, he made the Indonesian out to be an untried and brash newcomer; if he pointed to his historic place in colonial society, he implied that the Indonesian was traditionally a hewer of wood and a drawer of water.<sup>23</sup> Yet against Indonesian competition the Indo was fighting a lost battle. As Sutan Sjahrir, the outstanding Indonesian nationalist leader wrote: <sup>24</sup> "The Indos gradually become Indonesians, whether they want to or not . . . even if they would retain their 'European' status legally, the process of social transformation is making them one with the Indonesians, of whom many more will be academically trained in the near future, than all Indos put together." Their inability to understand the consequences of this "Westernization" of the Indonesian put the Indo in the peculiar position that he was in, argued Sjahrir: "they get the pressure from both sides and as a result they are all a bit twisted psychologically."

The Japanese occupation of Indonesia greatly amplified the Indo problem. Early in 1942 the Japanese introduced a racial classification system for Indos, based on the racial origin of their fathers: (1) a European father with an Indonesian or Eurasian wife (2) an Indo father with an Indo, European or Indonesian wife and (3) an Indonesian father with a European or a Eurasian wife. Every Indo was expected to prove his descent.<sup>25</sup> Most Indos who were offspring of unions in the last two categories were given a choice: they could either side with the full blood Dutch and be certain of a harrowing existence in some concentration camp or they could renounce

their European status, become Indonesians and *ipso facto* supporters of Japan's "Greater Asia Co-Prosperity Sphere." Indos with parents in the first category were virtually automatically deemed unsafe and imprisoned; how many in the other categories elected to remain—or perhaps "become"—Europeans is not known. It is certain that thousands of Indos decided to become Indonesians for the time being and not a few openly collaborated with the Japanese. As the war wore on the Japanese adopted an increasingly hostile attitude toward the Indo group, accusing it of retaining its "European" outlook.<sup>26</sup> Toward the end of the war hundreds of Indos were arrested. As usual the Indos as a group were mistrusted by all concerned: by the Japanese because of their suspected European loyalties; by the Europeans because of their collaborationist behavior, and by Indonesians because of their pre-war conservatism and hostility to nationalist aims. This distrust as well as the Japanese classification system undoubtedly increased the psychological tensions within the Indo minority. Colonial society had put a premium on European status and in that society many Indos had learned to ward off any threatening "Indonesianization" at all possible costs. Now admission of European status was equal to placing one's life in jeopardy, while in choosing the despised Indonesian status lay relative safety.

In the period of revolution following Japan's defeat this new orientation of the Indo's position continued. The leaders of the Indonesian Republic set great store by national unity among all population groups. But meanwhile scores of Indos were molested and murdered by Indonesian extremists who apparently regarded the despised *blanda item* (literally "Black white man"—a common Indonesian epithet for the Indo) as even more dangerous than the Dutch.<sup>27</sup> Some Indos, among them the redoubtable Douwes Dekker who had assumed an Indonesian name and had become a member of

<sup>23</sup> Koks, p. 246.

<sup>24</sup> Sjahrizad, *Indonesische Overpezingen*, Amsterdam, 1946, p. 138.

<sup>25</sup> Johan de Linde, *Anonè. De Europese gemeenschap gedurende de Japanse bezettingstijd*, Rotterdam, 1946, p. 24.

<sup>26</sup> D. H. Meyer, *Japan wint den oorlog. Documenten over Java*, Maastricht, 1946, pp. 40, 69. See also W. H. Elias, *Indië onder Japanschen Hiel*, The Hague, 1947, pp. 107-111.

<sup>27</sup> *Report of the Parliamentary Commission (States-General) Dutch East Indies*, The Hague, 1946, pp. 35-36.



the Republic's cabinet, urged all Indos to Indonesianize themselves and to abandon their class consciousness. Such views were in line with Republican policy. During a conference of Indos in February, 1947, held in the Republic's capital, Djokjakarta, Dekker maintained that it was "a psychological error" on the part of Indos to think that they were different from Indonesians. In the new Indonesia, according to Dekker, there should not be any minorities, who could become a pretext for aggression by some foreign power, i.e. the Dutch. The only solution to the Indo problem was assimilation: "Think Indonesian, become Indonesian, act Indonesian."<sup>28</sup> This appeal attracted only a minority of Indos at the time however, most Indos who had been incarcerated during the war sought the safety of the Dutch controlled coastal cities upon their release and there a revived IEV openly urged Indos to proclaim their loyalty to the Dutch cause. As tales of revolutionaries molesting Indos spread, the anti-Republican sentiment among the Indos rapidly increased and thousands of them took an active part in the military actions of the Dutch against the Republic. Others left the country. Early in 1947 the melancholy exodus of Indos to the Netherlands began, their numbers swelling by the hundreds, until by 1953 some 100,000 had gone to Holland where their presence caused severe socio-economic problems for the struggling post war governments.

Even before December, 1949 when the Netherlands formally transferred her sovereignty over the Indies to the Indonesian Republic and the latter became an independent state, three possible solutions of the Indo problem had been suggested. The first places its faith in the eventual absorption of Indos in the Netherlands. Indeed scores of the better trained Indos who have migrated to the Netherlands have had no difficulty in finding jobs and adjusting themselves to life in their nominal fatherland, which not a few of them had never seen before. It is to be feared that they are in a minority however. For years thousands of less fortunate Indos who had migrated had been forced to live in camps on a mere

government pittance; neither adequate housing nor sufficient jobs were available. Furthermore, while in Holland society is more democratic and less caste conscious by far than one time colonial Dutch society, there has been some resentment of and discrimination against the *lipper* (an uncomplimentary epithet for the Indo in use among full bloods). To be sure, Holland has for centuries been familiar with retired Indies hands, many of them Indos, who in such cities as the Hague and Amersfoort, preserved something of their Eurasian and colonial style of living. But they were generally wealthy and not present in such large numbers, nor were they generally of the same class as the majority of the present Indo immigrants, many of whom seem to have more pronounced "Eurasian" features, speak a broken Dutch dialect and have had less education than the old timers. For most of the Indos now in Holland, Europe is something learned out of a textbook, neither the climate nor the living conditions suit them. And finally there is the fact that even without the Indo minority Holland is a severely overpopulated country, where hundreds of native born seek emigration visas annually. The poverty stricken Indo immigrant merely swells the labor market, and since his training has in the majority of cases not been on a par with that of the average Hollander, he is inevitably becoming a public burden, a hindrance to the post war reconstruction of the country.

The second approach to the problem entails mass migration of Indos to new colonization areas. One such area that has been suggested is Western New Guinea, or Irian. Control over Irian is still in the hands of the Dutch, though Indonesian leaders have indicated that they will not rest until the area is part of their country. Negotiations concerning the future status of Irian have dragged on for years and have been punctuated by failure, all of which has greatly strained relations between the two countries. At present a little under four thousand Indos have migrated to Irian to lead a pioneer existence in the development of the area. Private pressure groups and business interests in Holland support the idea, but the Dutch government has been reluctant to give Indo colonization of Irian its

<sup>28</sup> G. W. Overdijkink, *Het Indonesische Probleem. Nieuwe Feiten*, Amsterdam, 1948, pp. 56-57.



active assistance. One reason is the strain in the relations with Indonesia. Another, perhaps more important, reason is that pre-war Indo colonization efforts in Irian, sponsored by the IEV in conjunction with the colonial government were complete failures.<sup>29</sup> Further, the actual resources of the area have been greatly exaggerated and it is a question of whether all, or even a majority of Indos, are suited for the rough and tumble of life in the bush. Those Indos that have gone to the area since the end of the war have little to recommend them at present except their tenacity; thus far their "colonization" has been unrewarding and badly planned and not a few have requested return passage home. Finally a noticeable reluctance to leave their European surroundings, however difficult at present, for a socially uncertain if economically better "mestizo" existence elsewhere has been evident in the ranks of the Indo immigrants. European status and class consciousness are often too deeply ingrained in them to make them willing to abandon its psychological security for an unknown environment outside Europe.

After 1949 the problem of the Indo immigrant became so acute that the Dutch government cast about for other colonization areas in which to settle them. Brazil has been suggested as one area, and a few Eurasians have gone to that country both before and since the war. Early in 1953 the government completed arrangements for the mass deportation of thousands of Indo immigrants to French New Caledonia, and the first of these have already left for the southwest Pacific. Others are contemplating settlement in the New Hebrides and in the West Indies. Immigration to such areas is hampered in the main by two factors. First there is the question of expense, not just the expenses of transport but also of providing the immigrants with funds to enable them to make a new start. The measures taken by the Indonesian government to combat inflation have led to severe curtailments of the export of capital from Indonesia, including accumulated pen-

sions and savings. The Indo migrant has no reserve to fall back on. The second factor is a racial one: "nearly all the more popular pioneer lands have rigid colour bars which effectively exclude Eurasians."<sup>30</sup> The peculiar class orientation of the Eurasian makes it essential for him to live in a country where his mixed racial origin will not, as in the case of colonial Indonesia, become a major social liability.

The third and last solution is based on the assimilation of the Eurasian in Indonesian society. About 100,000, or a little over half of the pre-war Indo group which regarded itself as Indo, or had been classified as European by colonial law, still remain behind in the Indies, most of them voluntarily. They include an indispensable small section who hold top positions in the administration and in business. The far greater majority of the remainder are those for whom a transition to an "Indonesian" outlook presents no great difficulty, since they were members of the two bottom levels of the pre-war Indo society, i.e. the paupers and the lower elements in the class of petty officials and subalterns. The middle stratum, about 30 per cent of the group, is placed in the most difficult position of all, however; among them the prestige factor of European status—often taken for granted by the Indo élite—is strongest, and resistance to assimilation with the Indonesian has been branded into them for generations. Some will eventually succeed in leaving the country, thanks also to the pressure exerted by their class-conscious fellows in the Netherlands, but for most there will through various circumstance be no such opportunity. Their only hope will lie in a psychological adjustment, an assimilation of mind and culture, as well as an eventual racial amalgamation.

The objections that have been raised against the continued existence of the Eurasian in Indonesia and to his eventual assimilation do not appear to be very valid. On the one hand it has been argued that leaving the Indos in Indonesia will "irrevocably mean, sooner or later, an 'Asiatic' living standard" for them, a debasement of

<sup>29</sup> On these early colonization schemes see J. Winsemius, *Nieuw Guinee als kolonisatiegebied voor Europeanen en van Indo-Europeanen* (Dissertation; Amsterdam, 1936).

<sup>30</sup> Charles Fisher, "Eurasian resettlement in Indonesia," *Eastern World* (October, 1951), p. 11.

the Indo's worth to himself and to society.<sup>31</sup> The fact of the matter is that long before the Second World War perhaps a third of the Indo group already had an "Asiatic" living standard: in 1934, for example, Indo clerks could be had for 10 guilders a month, and Indos trained at the Technical Institute were willing to work for the "Asiatic" wage of 4 cents an hour.<sup>32</sup> In the lower two levels of Indo society a standard of living was generally the rule which was certainly not that of the full blood European, but rather that of the skilled and semi-educated native Indonesian. While it is true that since attaining independence Indonesia's economy has been in a precarious condition and that the country will, as a whole, have to put up with living standards below those of before the war, it should also be noted that the Indonesian government is willing and able to pay well for trained personnel, and the same holds true for private enterprise. Since Indonesia has a dearth of trained technicians and well educated administrators, the Indo, by virtue of even the little training he has had, is often in a position to keep an economic position superior to that of the average Indonesian: in the land of the blind, the one-eyed man is king. With the development of Indonesia's productive resources, a better living standard is in the offing, not only for the Indonesian, but also for the Indo group. By identifying himself with the growing class of educated Indonesians, and not with the remnants of the Indo-European elite, the Indo can be adequately absorbed in the expanding economy of the country. But the *sine qua non* is again that he must regard himself as a son of the country, a citizen of a growing young republic, and not as a member of a grieving, alien minority.

Another objection to Indo assimilation that has been raised is that as a result of independence Indonesia will revert back to an "Indonesian" culture pattern, that "the former synthesis of East and West is a thing of the past," and that "the Eurasians, as the very embodiment of that synthesis

are placed in a cruel dilemma."<sup>33</sup> Such a view can only be maintained at the cost of ignoring the entire process of cultural change now underway virtually everywhere in Indonesia, which puts a premium on a dynamic adjustment of the "native" element to modern twentieth century civilization and on a large scale Westernization of government, social relations, the family, religion, art and cultural standards.<sup>34</sup> It also ignores the pronounced "Western" orientation of most modern Indonesian leaders. Far from placing "East" and "West" in antithetical positions and crushing the Indo between them, Indonesia is seeking to blend them. In the words of Supomo, president of the University of Indonesia:<sup>35</sup> "we should refrain from using the terms 'West' and 'East' as two 'opposite sides' in the same way as black and white, but what we should do is accept all that is good from the West and from the East." And far from returning to a traditional Oriental social order, in which the Indo's position would indeed be untenable, Indonesia seems to be heeding the words of its vice-president, the veteran nationalist Mohammad Hatta:<sup>36</sup> "Our world view is not static . . . but *historic-dynamic*. Therefore historic thinking should be the objective in the education of our youth, the thinking which includes the conviction that every thing will pass, that nothing is permanent." In such a "historic-dynamic" process of adjustment and change the Indo can plan an indispensable role in the synthesis of East and West in the country. In the past his function in this connection has often been unique. One is reminded, for example, of the *Stamboel*, the popular Indonesian opera, invented by a Eurasian, who translated the score of European operas into Malay, transposed the music often in a most original way and used Indonesian characters. *Stamboel* was

<sup>33</sup> Fisher, p. 10. For a similar sentiment see D. J. M. Kleymans, *Het Trojaanse Paard*, Dissertation, Utrecht, 1948, p. 168.

<sup>34</sup> See J. M. van der Kroef, "Patterns of Western Influence in Indonesia," *American Sociological Review*, XVII (1952), pp. 421-430.

<sup>35</sup> In the *Times of Indonesia*, Djakarta, Indonesia, December 12, 1952.

<sup>36</sup> Mohammad Hatta, *Verspreide Geschriften*, Amsterdam, Djakarta, 1952, p. 581. Italics in original.

<sup>31</sup> J. Schoonhoven and K. C. Snijsheuvel, *De Indo-Nederlandsche Volksgroep in de branding*, The Hague, 1951, p. 16.

<sup>32</sup> Furnivall, p. 435.

immensely popular among all population groups in the nineteenth and early twentieth century, a unique form of cultural synthesis of Eurasian origin.

Despite pessimistic predictions to the contrary there are definite signs that the Indos are making an adequate adjustment to the new social and political realities in Indonesia. For example in 1949 a new Eurasian group was founded, the *Partai Indo Nasional* with a program that demanded recognition for the Indo minority as full fledged *warga negara* (citizens) of the Indonesian Republic, as one group among many population groups loyally supporting the national state. While recognizing the peculiar position of the Eurasian, the party proclaimed Indos to be Indonesians and even went so far as to advocate the ultimate inclusion of Irian into Indonesian territory.<sup>37</sup> Indos have succeeded in securing leading positions in government and private enterprise and far from being regarded as an untrustworthy minority there are indications that other population groups are beginning to look on them as belonging to the country. Daily the process of peaceful assimilation goes forward.

There is every reason to believe that that process will continue. For insofar as the Indo in Indonesia was a problem, he was a colonial problem, one of the unhappy

aspects of an artificially created and socially enforced stratification and caste consciousness called into existence by the very nature of Dutch control. Where the pressure to be a member of the European élite has disappeared the social-psychological tensions will decrease and the "chip on the shoulder" attitude necessary to assert one's "European" status will become meaningless. The racial connotation of Indo status has vanished with the decline of blond haired, white skinned, full blood superiority in society. There is now no longer any racial criterion that sets the Indo apart. Many Indos look like full blood Indonesians, indeed some are even darker skinned than the Indonesians themselves. The only criterion that will remain is that of "Indo consciousness," so long as they feel themselves a minority apart, the Eurasians will be treated like one. But if the desire for assimilation exists the Indonesian government has the task of eliminating all vestiges of discrimination against the Indo, of absorbing him without regard for erstwhile status into all branches of government and enterprise on an equal footing with other nationals. Then and only then will the Indo problem be solved and will the day dawn, also for the Indo, of his awakening out of "the bad dream of having belonged to 'God's stepchildren,'" and will he become "a child of his own country, a man among other men."<sup>38</sup>

<sup>37</sup> *Kepartaian di Indonesia*, Kementerian Pengerangan Republik Indonesia; Djakarta, 1951, pp. 374-377.

<sup>38</sup> Wertheim, *Het Sociologisch Karakter*, p. 16.

## AN INVESTIGATION OF THE DISTINCTIVENESS OF SOCIAL CLASSES \*

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THIS study is an examination of social position in relation to social stratification. It poses the question whether American society consists of a few fairly discernable social strata or of a multitude

of overlapping social positions. The approach to this problem is by means of a simple profile scale which is designed to avoid forcing responses into predetermined categories. Where the majority of social class studies have requested the respondents to identify themselves with one of a set number of classes, this scale abstains from

\* Paper read at the annual meeting of the Society, Berkeley, California, August 30, 31 and September 1, 1953.



any reference to specific classes. Its main purpose is to reveal the actual pattern of social class structure.

Previous attempts have been made to construct a method whereby persons could be placed functionally within the social structure. A major limitation of most of these studies lies in the initial procedure. After the investigators have derived a picture of the community's class structure through careful observation, and have thus subjectively determined the number of classes, the next step has been to place the community members within their proper classes. It is obvious, however, that the general results may thus be predetermined by the investigator. It should be pointed out that most authors define structure only in order to get to the far more important psychological traits characterizing each class. But as in any social study of the survey type, representativeness hinges upon the manner in which the sample is selected. The lack of agreement between the findings of existent works is striking. The Useems and Tangent found three distinct social classes, Centers found four, Hollingshead found five and Warner found six.<sup>1</sup> In view of these discrepancies it is quite important that we ascertain whether our contemporary society consists of clearly delineated classes, or of a composite of statuses and roles arranged loosely along a continuum.

The present study applies profile scales which are exactly one foot in length. To reduce bias each scale was given only two reference points. One end read "The person with the highest . . . in your town", while the other end was labeled "The person with the lowest . . . in your town." Thus, using the community as a frame of reference, the respondents were asked to indicate their position on each scale by a pencil mark. For scoring purposes the scale

was divided into twelve inches with each inch representing a separate category.

It is to be logically expected that a scale of this type would tend to bias responses in the direction of its center point. However, by a method too involved to describe here, this was not found to be the case. As for reliability, when a pretest was performed on forty-five university undergraduates the coefficient of correlation obtained by the test-retest method was .91. This was sufficiently encouraging to warrant the use of the scale in a full community study.

The final instrument consisted of nine scales and an appendaged background data sheet. These scales were given in the following order: 1. Dwelling Unit; 2. Family Wealth; 3. Personal Influence; 4. Family Background; 5. Personal Income; 6. Social Position; 7. Occupational Prestige; 8. Social Class; 9. Residential Area (quality of neighborhood housing). The individual was instructed to rate his position within the community in terms of each of the nine foregoing criteria.

Neither social position nor social class can be considered as single variables. Nonetheless, they afforded the individual an opportunity to make a summary rating and gave the investigator a basis for analysis.

The social class scale was added because it was reasoned that if the population failed to fall into structured groups on the social position scale, then a social class scale should permit it another opportunity to do so.

The universe selected was the city of Washington Court House, Ohio. The 1950 census listed its population as 10,460, of which number 7 per cent were Negroes. It is a predominantly agricultural town, although its numerous small industries and well-balanced economy make it at least somewhat comparable to the larger cities. It is relatively isolated from the influence of large metropolitan areas. The population sampled consisted of all the white, voting-age adults within the city and its environs. A random sample was taken, containing an equal number of males and females. The survey was executed by two field workers during the first three months of 1952. All data were obtained by personal interviews. Each respondent was guaranteed complete anonymity.

<sup>1</sup> John Useem, Pierre Tangent, and Ruth Useem, "Stratification in a Prairie Town," *American Sociological Review*, 7 (1942), pp. 331-42.

Richard Centers, *The Psychology of Social Classes*. Princeton, New Jersey: Princeton University Press, 1949, Chap. II.

A. B. Hollingshead, *Elmtown's Youth*. New York: John Wiley, 1949.

Lloyd W. Warner, M. Meeker, K. Eells, *Social Classes in America*, Chicago: Science Research Associates, Inc., 1949.

After leaving the premises, the investigators rated each informant on three six-point scales. Each scale contained six categories weighted from one to six. The ratings entered upon these scales were admittedly arbitrary. They were not based upon highly standardized criteria and were at best a record of the observer's opinion. The factors so rated were the estimated social position of the family, the house and the neighborhood. The family was rated upon the amount and type of literature within the household, cleanliness, type of furnishings and general deportment of household members. The respondent's house and neighbor-

hood were rated in terms of appearance, material condition and apparent value. order of relationship might indicate that two different variables were involved. Social class is more consistently related to factors denoting group affiliation while social position appears to be more of a sphere of individual "power dominance". For instance, personal income and personal influence show less correlation with social class than with social position, and conversely, family background is more important to social class. A scattergram, not reproduced here, shows a normal distribution for both variables.

Of the rating scales used in this study, the four having the highest coefficients were occupational prestige, family background,

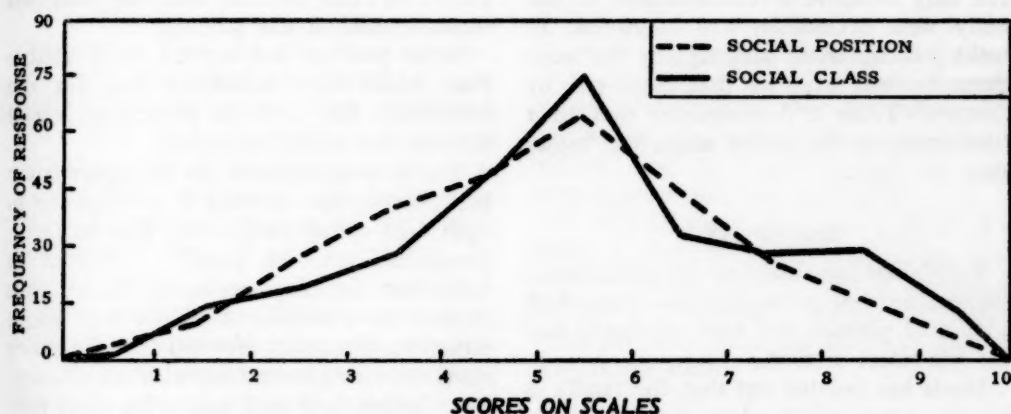


Fig. 1. DISTRIBUTION OF SCORES ON SOCIAL POSITION AND SOCIAL CLASS SCALES (N=300)

hood were rated in terms of appearance, material condition and apparent value.

The results of both the social position and social class scales are displayed graphically. It was necessary to contract the data to ten intervals in order to reduce random fluctuations between adjoining classes. Although not perfectly symmetrical, both graphs generally assume the unimodal character of a normal distribution curve. It is believed that if social classes have the characteristic form or forms commonly imputed to them, this unimodal distribution would not appear.

Social class and social position were correlated with the seven rating scales by means of the Pearsonian method. The results, in Table 1, show that the seven scale factors vary considerably in degree of correlation with social class and social position.

The correlation of social position with social class is only .65. This rather low

residential area and personal influence. They stand highest in relation to both social class and social position but in varying order. The multiple coefficient of correlation of these four scales with social class is .75; with social position it is .68.

The expression of these relationships has no finality because we are merely balancing one set of subjective criteria against another.

TABLE 1. CORRELATION COEFFICIENTS OF SOCIAL CLASS AND SOCIAL POSITION WITH RESPONSES TO OTHER SCALES

Rating Scales	Social Class	Social Position
Occupational prestige	.69	.57
Residential area	.54	.46
Family background	.53	.48
Personal influence	.49	.52
Dwelling unit	.47	.39
Family wealth	.45	.45
Personal income	.34	.44

TABLE 2. CORRELATION COEFFICIENTS OF OCCUPATION AND EDUCATION WITH RESPONSES TO RATING SCALES

Rating Scales	Occupation	Education
Social class	.42	.47
Social position	.35	.38
Occupational prestige	.43	.39
Dwelling unit	.41	.40
Residential area	.38	.37
Family wealth	.37	.34
Personal influence	.37	.28
Personal income	.25	.27
Family background	.23	.24
Education	.48	...

The only objective factors available to this study were occupation and education. In ranking occupations, substantially the same hierarchy was used as that employed by Centers.<sup>2</sup> Table 2 demonstrates that their relationship to the rating scales was rather low.

#### RELIABILITY

Reliability was tested by two procedures; the matched responses of spouses (for which additional samples had been obtained) and the test-retest method.

Davis has pointed out that the family is a unit across which class lines, of functional necessity, do not cut.<sup>3</sup> Therefore, if the class structure is well defined the status perspectives of husband and wife should be quite similar. The agreement between spouses, however, was not as close as would be expected according to the above conception.

Perhaps sociologists using wholesale methods of classification have overlooked the importance of individual psychological characteristics. For example, where the wife is dominant and socially active and the husband submissive and retiring one might well expect to find a difference in status perspective.

Fifty random cases were taken from the whole sample and retested after a lapse of twenty-one days. The difference between the correlation coefficients of reliability ob-

<sup>2</sup> Richard Centers, *The Psychology of Social Classes*. Princeton, New Jersey: Princeton University Press, October 1949, p. 48.

<sup>3</sup> Kingsley Davis, *Human Society*, New York: The Macmillan Company, 1950, p. 364.

TABLE 3. COEFFICIENTS OF CORRELATION OBTAINED BETWEEN HUSBANDS AND WIVES ON ALL SCALES (N=68)

Personal Rating Scales	Correlation Coefficient
Dwelling unit	.61
Social class	.49
Family wealth	.46
Residential area	.43
Social position	.40
Occupational prestige	.33
Personal influence	.29
Family background	.23
Personal income	.11

tained by this method and the matched response method was striking.

Social position was slightly more reliable than social class, indicating that for the individual, the variables underlying social position are more consistent.

The only explanation for the discrepancy between the two methods is a difference in individual status perspective. The fact that perspective may be largely a product of individual learning makes a strong case against the existence of tightly knit social structure. The retest revealed that the scales were measuring something with consistency, but whether they were measuring what they purported to measure is still unknown.

#### VALIDITY

Thus far, the only objective criteria brought to bear upon the subjective matrix were occupation and education. If our social structure is crystallized into class groups, these groups must have their roots in the community. What more objective evidence

TABLE 4. COEFFICIENTS OF CORRELATION OBTAINED BETWEEN THE ORIGINAL TEST AND RETEST (N=50 Cases)

Rating Scales	Test-Retest r
Social position	.81
Family background	.81
Occupational prestige	.80
Personal influence	.80
Residential area	.78
Social class	.75
Dwelling unit	.71
Family wealth	.69
Personal income	.54



ot their existence is possible than an expression of recognition by community members?

With the assistance of a five-man board, five judges were selected. Each judge came from a different social stratum and was a life-time resident of the community.

Once selected, each judge was presented with the names of the three hundred people in the sample. By a process of elimination thirty-five names were chosen. These thirty-five were extremely well known by all the judges. The investigators' six-point scale served as a rough guide in insuring that those persons thus selected were distributed along a social continuum.

The identity of each judge was kept anonymous. They did not sit as a panel, but were interviewed and instructed separately. Each judge was then allowed as much time as he required to score each person on four profile scales of the same type as those used in the main body of the study. The social class scale was omitted on the grounds that it might structure the judges' ratings.

When the judges' ratings were correlated with the respondents' self-ratings, the following coefficients were obtained: Social Position .47; Family Background .40; Family Wealth .39; Personal Influence .33.

Some support appears to be given to Hollingshead's assertion that the members of different social strata tend to rate other strata differently.<sup>4</sup> Each judge was most in accordance with the socially adjacent judges and the two judges at opposite social extremes disagreed the most sharply.

If the investigators' scale is acceptable as a measure of social position, it is interesting that the coefficient of correlation of the investigators' ratings with those of the respondents is identical to that of the judges with the respondents (.47). The fact that the investigators' ratings concur with concrete data is shown in its relationship to the respondents' occupation ( $r = .62$ ) and education ( $r = .58$ ). How was it that the observer, after brief and casual observation, was able to attain the same accuracy in rating the subject that the judges did? The judges were selected life-time residents, whereas the investigators had no prior acquaintance with the community.

It would appear that the opinions of a panel of judges, which have often been used as validity criteria in social class studies, are of doubtful efficacy, particularly when the phenomena sought are in such an inarticulate state.

<sup>4</sup> A. B. Hollingshead, *Elmtown's Youth*. New York: John Wiley, 1949, pp. 152-154.

## NEIGHBORHOOD REACTIONS TO ISOLATED NEGRO RESIDENTS: AN ALTERNATIVE TO INVASION AND SUCCESSION

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THE maintenance of residential segregation under a caste or semi-caste social system has resulted in a now familiar pattern of change when the minority group expands its numbers due either to immigration or to natural increase.<sup>1</sup> The process

begins when the area of minority dwelling increases its population density much beyond that of adjacent areas of majority dwellings. Since relatively little new building goes on in the old areas where minority groups live, the process of increase in population density occurs by means of doubling up of families in existing dwellings and conversion of older large units into several smaller ones. The one-room kitchenette apartment is the characteristic end-point of this conversion process in the "Black Belt"

<sup>1</sup> The process has been documented by several researches. See, for example: Herman H. Long and Charles S. Johnson, *People vs. Property*, Nashville: Fisk University Press, 1947; Robert C. Weaver, *The Negro Ghetto*, New York: Harcourt, Brace, and Company, 1948.

of the large Northern cities where large scale immigration of Negroes from the South has been going on for several decades. Because of other aspects of the caste system, most members of the minority group have relatively low incomes and are not able to pay for new buildings or for complete and adequate conversion of old ones. The splitting up of large old apartments or large old houses in segregated minority areas provides small units at moderate prices for the largest possible number of people.

In this situation several economic factors combine to start the process of movement into adjacent areas then inhabited solely by whites. (1) Population density reaches a natural limit within the minority area; there are no more houses or apartments available for conversion and people with money to spend on rent have no place to live. (2) While the rent for the one-room kitchenette is relatively low per family, it provides an unusually high income for the owner of the apartment or building because of the small amount of space a unit requires. For example, a large apartment which originally rented for 50 dollars a month, when split into five apartments each renting for 20 dollars a month, nets a total rental twice as high after conversion as it did before. It therefore becomes highly profitable for potential landlords to seek other large units still available for conversion; these are to be found only in areas occupied by members of the majority group. (3) Because the minority group areas are old and heavily over-populated, they have a slum character. Some members of the minority group, not many but enough to have an economic influence, have sufficient incomes to afford much better housing in better areas if they were in free market competition.

The movement of minority persons into adjacent majority group areas usually begins when some member of the majority group, planning to move anyway, finds it highly profitable to sell to a member of a minority group, or a third person, a real estate agent usually, purchases in his own name a piece of property in the majority group area and turns it over at a very large profit to a member of the minority group. If all the members of the majority group

are highly cohesive they are sometimes able to eject the sole minority group family, but generally there are some persons who either believe that a trend toward minority group movement invasion is inevitable and are willing to sell out to other members of minority groups in order to leave the area, or become panicky and think their property will become nearly worthless unless they sell quickly, or are willing to take advantage of the still-high prices for sales to minority persons. After two or three houses are sold to members of the minority group the property begins to change hands rapidly as the resistance of the majority group crumbles. For a brief period, ranging from a few weeks up to a year or so, the property of the area may be sold very cheaply. Quite often members of the minority group seeking housing do not have sufficient cash available to buy these cheap properties but they are sold to real estate agents or other people who intend to make money out of real estate through conversion or resale. After the scare selling period the prices of property in the area of transition go up sharply until they reach the high level prevalent in the old minority group area and are considerably higher than those in equivalent areas occupied by the majority group.<sup>2</sup> And so the solidly ethnic Black

<sup>2</sup> Studies on the racial factors in property value movements include: Elsie Parker, "Both Sides of the Color Line," *The Appraisal Journal*, January 1943, pp. 27-34; July 1943, pp. 231-49. George W. Beehler, Jr., "Colored Occupancy Raises Values," *The Review of the Society of Residential Appraisers*, September 1945, pp. 3-6, 12. Paul F. Cressey, *The Succession of Cultural Groups in the City of Chicago* (unpublished Ph.D. thesis, University of Chicago, 1930). Oscar I. Stern, "The Long Range Effects of Colored Occupancy," *The Review of the Society of Residential Appraisers*, January 1946, pp. 4-6. Homer Hoyt, *One Hundred Years of Land Values in Chicago*, Chicago: University of Chicago Press, 1933. Richard Marks, "The Impact of Negro Population Movement on Property Values in a Selected Area in Detroit," unpublished study made for the Mayor's Interracial Committee of the City of Detroit, 1950. Egbert F. Schietinger, *Real Estate Transfers During Negro Invasion: A Case Study* (unpublished M.A. thesis, University of Chicago, 1948), 118 pp. Belden Morgan, "Values in Transition Areas: Some New Concepts," *The Review of the Society of Residential Appraisers*, March, 1952, pp. 5-10. Luigi M. Laurenti, "Effects of Nonwhite Purchasers on Market Prices of Residences," *The Appraisal Journal*, XX (July, 1952), pp. 314-329.

Belt, or the ghetto, or the Mexican district, expands.

In the last few years, however, new factors have entered the situation which are changing this pattern of "invasion and succession."<sup>3</sup> One factor is the change in attitude toward living near members of a minority group. Partly as a result of an organized effort to reduce prejudice and discrimination, and partly as a result of the housing shortage, more people are "willing" to live next door to members of minorities than was formerly the case. The second new factor is a Supreme Court decision of 1948 which removed the legally enforceable basis of the restrictive covenant, which has hitherto been the most powerful legal device used to prevent members of the minority groups from buying or renting in majority group areas. Since 1948, in most Northern cities and some Southern cities as well, Negroes have been moving into many white neighborhoods which under the old system would have taken them decades to penetrate, if ever they could have gotten in at all. There are now a large number of otherwise white neighborhoods into which one or two Negro families have moved. Mixed housing seems to be becoming the dominant pattern in at least the Northern cities.

What are the social consequences of this new pattern of Negro-white living in American cities? How do the whites react to Negro neighbors? One answer comes from studies of government-subsidized housing

projects in which apartments are made available only on an unsegregated basis.<sup>4</sup> This type of study, however, was not designed to answer our questions. While suggestive, it has certain defects in indicating the social consequences of a general breakdown in residential segregation because: (1) the people involved are drawn mainly from the lower income classes; (2) the situation is one in which all of them are placed simultaneously in the neighborhood rather than one in which whites see a few Negro families entering "their" neighborhood; (3) there is a much more obvious source of pressure for unsegregated living in the projects, since there is government subsidy and government direction, whereas in an ordinary community the unsegregated living seems much more voluntary—being limited only by such impersonal factors as the housing shortage and inertia against moving.

Another type of research which would aid in determining how whites react to Negro neighbors in the newly developing pattern of mixed housing is a study of communities where one or two Negro families have been living for some time among a much larger population of whites. Such communities are found in Minneapolis, where our study was conducted. Minneapolis has otherwise a pattern of race relations not perceptibly different from that prevailing in other Northern cities. The one significant way in which Minneapolis differs from other Northern cities is in its proportion of Negroes: Only 1.3 per cent of the population of Minneapolis is Negro, as compared to 13.6 per cent in Chicago, 16.2 per cent in Detroit, 9.5 per cent in New York, and 18.1 per cent in Philadelphia. Other Northern cities have smaller proportions of Negroes. Whether this small proportion in Minneapolis makes it non-comparable with other cities is not

<sup>3</sup> These are terms borrowed by Robert E. Park from plant ecology to describe the analogous human process we are considering. Robert E. Park, "Human Ecology," *American Journal of Sociology*, XLII (July, 1936), pp. 1-15; "Succession, An Ecological Concept," *American Sociological Review*, I (April, 1936), pp. 171-179. Also see: Bessie McClenahan, *The Changing Urban Neighborhood*, Los Angeles: University of Southern California, 1929, pp. 5, 26-29, 83-89. While Park used the term "invasion" in an ecological context of competitive relationships, the term is an unfortunate one since its popular meaning has a strong connotation of conflict. In its use by realtors, newspaper writers, and other non-sociologists—although they have learned the term from sociologists—"invasion" has a moral connotation of unrighteous seizure of property from unwilling sellers. The term has probably helped transform a competitive process into a conflict process.

<sup>4</sup> A carefully designed research of this type, involving comparison of housing projects that are completely unsegregated with projects that have Negroes living in segregated sections, is that of Morton Deutsch and Mary Evans Collins, *Inter-racial Housing*, Minneapolis: University of Minnesota Press, 1951. This study has been followed up and its results are confirmed in a study by D. M. Wilner, R. P. Walkley, and S. W. Cook, "Residential Proximity and Intergroup Relations in Public Housing Projects," *Journal of Social Issues*, 8 (1st issue, 1952), pp. 45-59.



known, but it needs to be stressed that in every other aspect of race relations Minneapolis does not differ from other Northern cities. There is a small "Black Belt" in Minneapolis, but a few Negro families have managed to obtain residences in most other parts of the city. Some moved into the otherwise white neighborhoods many years ago, while others moved in fairly recently, and we shall compare these two types of neighborhood. In nearly every case there was some opposition to the Negro family moving in, and in one or two cases there was actual violence. But in all the cases we shall consider, the Negro family stayed and so did most of the whites (those whites who moved out were replaced by other whites). Other Negro families did not follow the first one into the neighborhood, since the pressure on Minneapolis Negroes to secure living space was not nearly as great as elsewhere, and consequently there was no process of "invasion and succession." This now seems to be becoming the new pattern for other large cities, at least in the North, as Negroes find it increasingly possible to move where they wish to, and not only into a Black Belt.

The data were collected by means of interviews, using a schedule with mainly check-list answers, conducted by volunteer students specially trained for the purpose. Interviews were conducted in the spring of 1951 in eight neighborhoods, chosen because they had only one Negro family (in one case, two Negro families) living in what was otherwise a white residential neighborhood. In four of the neighborhoods the same Negro family had been resident for at least ten years, while in the four others the Negro family had been resident less than two years. In each group of four neighborhoods, two were chosen as lower-income areas, and the two others as middle-income areas. The neighborhood was defined in terms of distance from the Negro's home and divided into a primary and a secondary zone: in the primary zone, consisting of the homes on both sides of the street within one block of the Negro's dwelling, one adult was to be interviewed within every home; in the secondary zone, containing all second blocks from the Negro's home on the same street,

as well as one block on all adjacent streets, every other dwelling was included in the sample. Deviations were made from this plan because of odd block structures, the presence of factories and railroad tracks, and other neighborhood barriers. A total of 545 interviews were obtained; of the total original sample 9.6 per cent was lost because no person could be found at home after repeated visits or because of refusal to be interviewed. Information was obtained on the personal characteristics of the white and Negro families, but these data can only be used to explain apparent exceptions in the attitude and behavior patterns, since not enough neighborhoods were studied to make comparisons between types of neighborhoods in terms of the personal characteristics of the residents.

Before the study began, a number of hypotheses were set forth and used to formulate the questions in the schedule. They fell into the following subject-matter areas: (1) satisfaction with and participation within the neighborhood; (2) attitudes toward and knowledge about the Negro residents; (3) the extent and kinds of relationships with the Negro residents; (4) general endorsement of interracial housing and association. The specific hypotheses will now be presented with the relevant data.

1. The closer neighbors have more contact with the Negro family and are more willing to approve of the general idea of mixed racial housing and association than are those persons who live at a greater distance from the Negro family. For all neighborhoods where Negroes had been living for at least ten years, 63 per cent of the white respondents in the primary zones speak with members of the Negro family, while in the secondary zones only 23 per cent had a speaking acquaintance with the resident Negroes. For neighborhoods where Negroes have been living for less than two years, 36 per cent of the whites living in primary zones speak with members of the Negro family as compared to 30 per cent in the secondary zones. This finding is consistent with that found in studies of areas that are racially homogeneous, that closer neighbors have more neighborly contacts

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than do relatively distant neighbors.<sup>5</sup> The measurement of approval of mixed racial associations was made by three questions whose answers were found to scale according to Guttman criteria.<sup>6</sup> These questions asked whether the respondent thought Negroes should be permitted to live in the same building,<sup>7</sup> to live in the same block, to go to the same school, as whites.<sup>8</sup> Table 1 shows that, for areas where Negroes have been living for at least ten years, respondents who live in the primary zones include a larger proportion in favor of interracial association than respondents who live in secondary blocks, but that for areas where Negroes have been living for less than two

ing getting along with the Negro family the results are mixed. On the one hand, a significantly greater proportion of the primary zone population than of the secondary zone population said that their relationships with the Negro families were very good (43 as compared to 10 per cent in areas where Negroes have been living at least ten years; 17 as compared to 11 per cent in areas where Negroes have been living less than two years); on the other hand the very few respondents who said they did not get along well were mostly also in the primary zone (3 per cent as compared to less than 1 per cent in both types of areas). The latter cannot be said to go against our hypothesis,

TABLE 1. ATTITUDE TOWARD INTERRACIAL ASSOCIATION IN PRIMARY AND SECONDARY ZONES

Those Living in	Number Responding to All 3 Questions	Percentages Taking Each of Following Positions with Respect to Interracial Association:				Mean Score
		0 Sch., Blk., Bldg.—No	1 Sch.—Yes; Blk., Bldg.—No	2 Sch., Blk.—Yes; Bldg.—No	3 Sch., Blk., Bldg.—Yes	
Areas where Negroes have been living at least 10 years:						
Primary zones	(79)	4	35	30	30	1.85
Secondary zones	(174)	14	40	23	22	1.52
Areas where Negroes have been living less than 2 years:						
Primary zones	(90)	13	37	17	33	1.70
Secondary zones	(159)	18	30	20	32	1.66

years there is no significant difference between those living in primary and secondary zones. When the question is raised concern-

since not getting along well with a specific family might reflect a reaction to a particular family rather than a reaction to Negroes in general.

2. Those who have more contacts with their Negro neighbors are also those who are more favorable to interracial association and who have a more favorable opinion of their Negro neighbors.<sup>9</sup> Some of the data already presented support this hypothesis. Whites who live in areas where a Negro family has been living for at least ten years, and live in primary zones close to the Negroes, both have more contact with their

<sup>5</sup> L. Festinger, S. Schachter, and K. Back, *Social Pressures in Informal Groups*, New York: Harper, 1950; T. Caplow and R. Forman, "Neighborhood Interaction," *American Sociological Review*, 15 (June, 1950), pp. 357-366.

<sup>6</sup> Louis Guttman, in S. A. Stouffer *et al.*, *Measurement and Prediction*, Princeton: Princeton University Press, 1950, pp. 46-90.

<sup>7</sup> Living in the "same building" is an ambiguous matter, since some respondents live in duplexes and apartment buildings, while most live in single-family homes.

<sup>8</sup> The exact wordings were: "Do you think that Negroes should be permitted to live in the same building with white persons?" "Do you think Negroes should be permitted to live in the same block with white persons?" "Would you object to Negro and white children going to the same school?" The answers permitted were simply "yes" and "no."

<sup>9</sup> This hypothesis gets support from many other studies, with certain qualifications. See Stuart W. Cook, "Contact and Intergroup Attitudes: Some Theoretical Considerations," Presidential address to Society for the Psychological Study of Social Issues, September, 1952.

Negro neighbors and are somewhat more likely to be favorable to them specifically, and to interracial association generally, than are whites who live in areas where a Negro family has been living for less than two years. Whites who live in primary zones (i.e. closer to their Negro neighbors) are more acquainted with their Negro neighbors than are whites who live in secondary zones, and also tend to get along with them better.

a more serious limitation of our data, however, due to the actually limited character of "neighborly" contact in a large city: There were so few kinds of contact with Negro neighbors (with other white neighbors also, in most cases) that, among the various questions on contacts we asked, the only one eliciting enough positive answers to permit a significant cross tabulation was the contact of "speaking with" the Negro neigh-

TABLE 2. RELATIONSHIP OF CONTACT WITH EVALUATION OF NEGRO NEIGHBORS, AND WITH ATTITUDE TOWARD INTERRACIAL ASSOCIATION

Question	Answer	Percentage Expressing Indicated Attitude Among Those Who, <sup>1</sup> in Areas Where Negroes Have Been Living			
		At Least 10 Years		Less Than 2 Years	
		Speak with Negro Neighbor	Do Not Speak with Negro Neighbor	Speak with Negro Neighbor	Do Not Speak with Negro Neighbor
"Disregarding their race, how well do you think this family compares as neighbors with the other families in this area?"	Better	16	6	19	8
	About the same	81	85	72	79
	Not as well	3	9	9	13
Number of cases		(92)	(89)	(67)	(109)
Scale of attitude toward interracial association	0 Sch., blk., bldg.—no	4	14	7	21
	1 Sch.—yes; blk., bldg.—no	32	44	23	35
	2 Sch., blk.—yes; bldg.—no	38	19	28	16
	3 Sch., blk., bldg.—yes	26	23	42	28
Number of cases		(92)	(154)	(72)	(153)

<sup>1</sup> Persons who indicate that they don't know their Negro neighbors are excluded from the top half of this table.

Primary zone whites are more favorable to interracial association generally, however, only if the Negroes have been living in the neighborhood for a considerable length of time. Our direct correlational evidence also gives support to the hypothesis. While correlational evidence does not, by itself, indicate the direction of causation, in the light of the above mentioned considerations it seems reasonable to assume that the contact is the "cause" and the attitude toward interracial association the "effect." There is

bor. Table 2 shows the relationship between this contact and evaluation of Negro neighbors and approval of interracial association. The differences showing the relationships are all consistent and all statistically significant at the five per cent level at least, but they are not very great.

3. The third hypothesis is that areas which are more integrated, and individuals within any area who are more integrated into it, will display a greater degree of unity in their response to the Negro family and



to interracial association in general. To measure integration, a scale was formulated in the pattern of a social distance scale,<sup>10</sup> except that social distance was expressed toward a neighbor rather than toward minority groups. When comparisons were made between areas with highest and lowest average scale scores, and between individuals with high and low scale scores within all primary areas, on several indices of association with the Negro family and with attitude toward interracial association generally, no consistent or statistically significant

the general social distance scale, for people who live in the primary zones of areas where Negroes have been living for at least ten years. It shows that people who are less socially distant from their neighbors in general are slightly more likely to know their Negro neighbors, and this acquaintanceship is more likely than not to take the form of a friendly relationship. The difference is not great (52.5 per cent among the socially distant who get along "very well" or "fairly well" with the Negro family, as compared to 61.6 per cent among the less socially dis-

TABLE 3. COMPARISON OF OWNERS AND RENTERS IN ACCEPTANCE OF NEGRO NEIGHBORS AND IN BELIEF THAT PRESENCE OF NEGROES DECREASES PROPERTY VALUES

Questions	Answers	Percentages Giving Indicated Response Among:			
		In Areas Where Negroes Have Been Living for at Least 10 Years		In Areas Where Negroes Have Been Living for Less Than 2 Years	
		Owners	Renters	Owners	Renters
"Do members of this family ever talk to you or your family?"	Yes	34	23	30	27
"Disregarding their race, how well do you think this family compares as neighbors with the other families in this area?"	Better	8	3	8	10
	Same	55	51	52	45
	Not as well	4	3	10	3
	Don't know them	33	43	30	42
"Some time ago a Negro family moved into this neighborhood. Do you think this hurt property values in the neighborhood?"	Yes	34	34	51	30
	No	36	32	32	41
	Don't know	30	34	17	30
Number of cases		(213)	(35)	(190)	(62)

differences in variation appeared. Thus the hypothesis is not supported by our data.

4. The fourth hypothesis is that those who are more integrated into the neighborhood, or who have a stronger "stake" in the neighborhood, are more inclined to accept the specific Negro family, where the Negro family has been living for a long time. The several relevant items of data in our study support this hypothesis. (a) A comparison was made of people with different scores on

tant) and the cases are so few (40 and 48, respectively) that the difference cannot be considered statistically reliable (at the 30 per cent level). (b) Ownership of property represents another index of integration into the neighborhood, and data relating ownership to acceptance of the Negro family are presented in Table 3. A larger proportion of owners than of renters are acquainted with the Negro families, and are as likely to evaluate them favorably as neighbors. In areas where Negroes have been living for at least ten years, there is no difference between the proportions of owners and renters believing that the presence of the Negro family lowers property values in the neighborhood, although a differential appears

<sup>10</sup> The principles underlying social distance scales have been formulated by Emory J. Bogardus. See his "Measuring Social Distance," *Journal of Applied Sociology*, 9 (1925), pp. 299-308; and "A Social Distance Scale," *Sociology and Social Research*, 17 (1933), pp. 265-271.

in areas where Negroes have been living less than two years. Owners are, of course, the ones directly affected by lower property values, and the fact that no more of them—in areas where Negroes have been living for ten or more years—believe that the presence of the Negro families decreases property values is undoubtedly a reason why they are as willing as renters to accept the Negro families.

5. Complementary to the preceding hypothesis that the more integrated people in the neighborhood are more likely to accept the specific Negro family is another hypothesis that they are more likely to resist

The specific responses of those dwelling in areas where Negroes have been living at least ten years show that the owners are more likely than renters to be more favorable as well as less favorable to interracial association in general. This is possible because owners are more likely to take the position that Negro children should be allowed to go to the same school as white children go to, but that Negro families should not be allowed to live in the same block or building that white families live in. Thus owners express both their friendliness to Negroes—which we suggest arises from their satisfactory relations with their Negro neighbors—as well

TABLE 4. COMPARISON OF OWNERS AND RENTERS IN ATTITUDE TOWARD INTERRACIAL ASSOCIATION

Position on Scale of Attitude Toward Interracial Association		In Areas Where Negroes Have Been Living 10 or More Years, Percentage Expressing Indicated Attitude Among:		In Areas Where Negroes Have Been Living Less Than 2 Years, Percentage Expressing Indicated Attitude Among:	
		Owners	Renters	Owners	Renters
0	Sch., blk., bldg.—no	10	19	18	9
1	Sch.—yes; blk., bldg.—no	43	25	36	22
2	Sch., blk.—yes; bldg.—no	24	28	18	19
3	Sch., blk., bldg.—yes	23	28	28	50
Mean score		1.59	1.65	1.56	2.10
Number of cases		(213)	(35)	(190)	(62)

a more general interracial association. Using the respondents of the primary zones only, we find that the average score on the scale of interracial association for the more socially distant people is 1.92, whereas for the less socially distant people it is 1.88 (based on 40 and 48 cases, respectively). This is not a statistically significant difference but it is in the expected direction. Table 4 shows a more conclusive and more interesting comparison between owners and renters. On the mean score, in areas where Negroes have lived at least ten years, owners are slightly less favorable to interracial association in general than are renters. The difference is in the direction which confirms our hypothesis but is not large enough to be statistically significant. In areas where Negroes have been living less than two years, the difference between owners and renters is sharply in the expected direction and is highly significant.

as their concern about the value of their property.

6. Children in a family create for their parents both a stronger relationship to the neighborhood—including Negro neighbors—and at the same time an anxiety about the effect of Negro neighbors on the children. Therefore we hypothesize that respondents with young children—who would create anxiety but are too young to create relationships—will be more distant from the Negro neighbors than are respondents without children, but that respondents with older children have counteracted their anxiety because of the increased relationship with the Negro neighbors for which the children were responsible. Table 5 provides striking confirmation of this hypothesis for areas in which Negroes have been living for at least ten years. Respondents with children of pre-school age are least likely to have re-

relationships with the Negro neighbors and most likely to be hostile to interracial association in general, whereas respondents with older children are much more likely to have contact with the Negro family and less hostility toward interracial association. Respondents with school age children, as compared to respondents without children, include a slightly larger proportion hostile

any other group. We might hazard the guess that the state of attitudes predicted in our hypothesis has not yet had a chance to take form in the short time Negroes have been living in the neighborhood.

7. Our seventh hypothesis is one that has several times been confirmed in earlier studies.<sup>11</sup> It is that the more educated the person the more likely he is to be favorable

TABLE 5. RELATIONSHIP BETWEEN PRESENCE OF CHILDREN IN THE FAMILY AND CONTACT WITH NEGRO NEIGHBORS, AND ATTITUDES TOWARD INTERRACIAL ASSOCIATION

In Areas Where Negroes Have Been Living at Least 10 Years:	Percentage of Respondents Giving Indicated Answers Among Those with:			
	No Children	Pre-School Children	School-Age Children	Children 19 and Over
Percentage speaking with members of Negro family	31	12	39	43
Position on scale of attitude toward interracial association				
0 Sch., blk., bldg.—no	8	23	13	16
1 Sch.—yes; blk., bldg.—no	41	53	31	40
2 Sch., blk.—yes; bldg.—no	27	13	22	20
3 Sch., blk., bldg.—yes	24	10	33	24
Mean score	1.66	1.10	1.76	1.52
Number of cases	(118)	(34)	(56)	(28)

In Areas Where Negroes Have Been Living Less Than 2 Years:	Percentage of Respondents Giving Indicated Answers Among Those with:			
	No Children	Pre-School Children	School-Age Children	Children 19 and Over
Percentage speaking with members of Negro family	24	28	27	22
Position on scale of attitude toward interracial association				
0 Sch., blk., bldg.—no	15	7	24	28
1 Sch.—yes; blk., bldg.—no	42	33	34	24
2 Sch., blk.—yes; bldg.—no	16	24	13	20
3 Sch., blk., bldg.—yes	27	35	29	28
Mean score	1.55	1.86	1.47	1.48
Number of cases	(86)	(57)	(40)	(27)

to all three types of interracial association as well as a larger proportion not hostile to any of the three types. In areas where Negroes have been living for less than two years, the hypothesis does not hold up at all. There are no significant differences in percentage of families speaking with members of the Negro family, and the major difference in attitude toward interracial association generally is that whites with pre-school age children are more favorable than

to interracial association but the less likely he is to have had contact with his Negro neighbor. Table 6 gives direct confirmation of this. Especially in attitudes toward interracial association there are striking differences between respondents of different educational levels.

<sup>11</sup> See the summary contained in: Arnold M. Rose, *Studies in Reduction of Prejudice*, Chicago: American Council on Race Relations, 2nd edition, 1948, pp. 19-24.



8. We had no hypothesis about the relationship between religious affiliation and attitudes toward interracial association, but a comparison of scores of different religious groups on the scale of attitude toward interracial association shows that the religious groups are not very far apart. For areas in which Negroes have been living at least ten years, the score of Catholics (52 cases) was 1.59, of Lutherans (106 cases)—1.55, of members of the other large Protestant denominations (51 cases)—1.55, of the fundamentalist Protestants (16 cases)—1.50, and

ency to accept or to accommodate to the Negro as a neighbor.<sup>12</sup> Those who live close to the Negro neighbors have more contact with them and are more favorable to them and to interracial association generally than those who live farther away. This is especially true in neighborhoods where Negroes have been living for a long time. Those who have more contacts with their Negro neighbors are more favorable to them specifically and to interracial association generally. Those who live in neighborhoods where Negroes have been living for at

TABLE 6. RELATIONSHIP BETWEEN EDUCATION AND CONTACT WITH NEGRO NEIGHBORS AND ATTITUDES TOWARD INTERRACIAL ASSOCIATION

In Areas Where Negro Family Has Been Living for at Least Ten Years:	Indicated Responses Among Persons with Educational Attainment at Level of:			
	Grade School Only	High School Only	Some College	College Graduation
Mean score on scale of attitude toward interracial association	1.40	1.74	1.97	2.04
Percentage who speak with Negro neighbor	36	36	29	29
Number of cases	(64)	(125)	(36)	(23)
In Areas Where Negro Family Has Been Living Less Than Two Years:				
Mean score on scale of attitude toward interracial association	1.43	1.59	1.83	2.31
Percentage who speak with Negro neighbor	30	35	24	40
Number of cases	(57)	(113)	(46)	(22)

of those who said they had no religious affiliation (29 cases)—1.73. For areas in which Negroes have been living for less than two years, the score of Catholics (62 cases) was 1.46, of Lutherans (97 cases)—1.72, of members of the other large Protestant denominations (38 cases)—1.92, of the fundamental Protestants (12 cases)—1.84, and of those who said they had no religious affiliation (26 cases)—1.63.

#### CONCLUSION

The evidence of this survey of eight neighborhoods of Minneapolis where a single Negro family lives in an otherwise white neighborhood indicates that there is a tend-

least ten years, and live close to the Negroes, are somewhat more likely to have contact with their Negro neighbor and to be more favorable to them and to interracial association generally than whites who live in neighborhoods where Negroes have been living less than two years. Those who are more integrated into the neighborhood and who

<sup>12</sup> In Duluth, virtually all Negroes live in neighborhoods that are predominantly white, and a survey by Turbeville reports that "over 82 per cent of the heads of households stated that they had not been victims of neighborhood discrimination because of their race." Gus Turbeville, "The Negro Population in Duluth, Minnesota, 1950," *Sociology and Social Research*, 36 (March-April, 1952), pp. 231-238.

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have a stronger "stake" in it are more inclined to accept the Negro family, although not to be more favorable to interracial association generally. We used home ownership—as opposed to renting—as an index of "stake in neighborhood" and it is significant that the owners—in areas where Negroes have been living ten or more years—were as favorable toward, probably because they were more acquainted with, the Negro family, and no more inclined to believe that the presence of the Negro family hurt property values. Renters are slightly more likely to be favorable toward interracial association in a neighborhood, however, while owners are slightly more likely to be favorable toward interracial association in the schools, in areas where Negroes have been living at least ten years. In areas where Negroes have been living less than two years, owners are less likely than renters to be favorable to interracial association and more likely to believe that Negroes hurt property values. Well-educated whites are much more likely to be in favor of interracial association, but

not more likely to have actually had contact with their Negro neighbors, than are poorly educated whites. In areas where Negroes have been living for a long while, families with school age children are more favorable to interracial association than are families with pre-school age children. This pattern has not developed, however, in areas where Negroes have been living less than two years. The dominant religion of the whites in a neighborhood does not seem to affect any of the above patterns, although data not presented suggest that the more heterogeneous the population, in terms of religion and national origin, the more favorable is the area toward Negroes.

In so far as the limited data of this survey of Minneapolis can be generalized, if the residential pattern of Northern cities takes the form of a scattering of Negro families living in predominantly white areas, the prognosis is that this would tend to increase the acceptance and accommodation of Negroes by whites.

## WORK SATISFACTION AND SOCIAL REWARD IN QUOTA ACHIEVEMENT: AN ANALYSIS OF PIECEWORK INCENTIVE \*

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THIS paper offers some observational and conjectural results of an attempt to probe the "reward composition" of piecework, a device commonly used in factory management to stimulate machine operatives to greater productive effort. A tentative answer to the question: Why does the piecework system succeed to the extent that it does? will be essayed here. What reward, or rewards, does piecework offer, in the perception of the man on the machine? What is the pieceworker responding to when he does intensify his efforts? Does piecework provide

an economic incentive, in that the operative sees and strives to grasp opportunity for a fatter pay check? Or does it offer rewards other than economic ones? Data that appear to bear upon this issue have accrued from the writer's own experience as participant observer on the production line of a piecework machine shop in a steel processing plant.

Impressionistic conclusions drawn after eleven months' experience on the drill line of the shop investigated might well indicate that any discussion of the nature of piecework incentive would be purely academic; for it would seem that no special inducements to work effort, economic or otherwise, were effectively operating. Machine operators, including lathe hands and milling

\* This report is drawn from the writer's doctoral dissertation, "Restriction of Output in A Piecework Machine Shop" (University of Chicago, 1952), under the direction of Everett C. Hughes.

machine men as well as the men on the drill line, were exhibiting under the assumed influence of piecework something considerably less than an all-consuming zeal for maximizing production. In fact, they showed keen interest in developing ways and means of curtailing output. The work group was applying a heavy foot to the production brake, day in and day out.<sup>1</sup>

Close scrutiny of the particulars of "soldiering," as recorded in a daily work diary, revealed certain differences in operator behavior that could be of significance. For one thing, loafing on the job was discovered not to be simple inactivity. Output restriction was found to include far more than the setting and observing of quotas. Machine operators maintained production, under certain conditions, at quota levels; but they also applied the brakes, under other conditions, at levels of production far below the quota. It was found that the distribution of operator hourly piecework earnings by jobs, or operations, was bimodal in character, suggesting two kinds of response to piecework. Classification of restriction into two major types, quota restriction and "goldbricking," with further classification of the latter into subtypes, was possible through the writer's opportunity to get a close view of work behavior.<sup>2</sup> Furthermore, machine operators did not loaf all the time. Sometimes, when exerting themselves to achieve quota, they expended effort to the maximum.

It seemed a simple exercise in logic, when these first discriminations were made, to account for such major differentiations in work behavior in terms of economic rationality. Machine operators exerted themselves to attain quota production, limited their output to quota ceilings, goldbricked at levels of output below hourly, or "day rate," pay standards when substantial premium earnings were felt to be unattainable, and loafed on non-piecework operations because in each case they were being alert to their economic interests. It appeared that supporting evidence could be drawn from explanations given by the workers themselves. Their constant reference to money in ex-

pressions of sentiment toward job assignments; their primary labeling of piecework operations as "gravy" jobs or "stinkers," based on possibilities of achieving premium earnings; their dislike of "day work," or the operations that carried no piecework price; their alternate apprehension, indignation, elation, in regard to the setting of piecework prices; their chronic concern over a wide range of financial problems in home and community relationships; in short, a general permeation of intra-group communication with economic messages of one sort or another would lead to the easy inference that here the economic motive was dominant. Twin conclusions that the partial success of the piecework system was due to periodic sufficiency of economic incentive, and that partial failure was connected with periodic insufficiency of economic reward, would seem to emerge almost automatically from the data.

However, the writer's attention was drawn to a few negative cases that seemed to deny the all-powerful influence of economic rationality and to point to a need for still closer examination of response to piecework. For instance, on some occasions operators either failed to put forth effort toward the attainment of quota earnings, when quota achievement was recognized as possible, or they stopped production at levels short of the quota line. Also, operators occasionally showed an indifference to maximization of net earnings, by means of a disinclination to work overtime, by quitting early after working partial shifts when jobs did not suit them, or by laying off to avoid receipt of non-piecework operations. In addition, the verbal behavior of operators at times indicated that they did not care primarily about the money. The alert eye and ear could catch contradictory bits in the flow of events and verbalizations not accountable for by a theory of economic motivation.

For example, Tony (all names used in this report are fictitious) would complain vigorously that slow delivery of materials to his speed-drill checked his money making; yet he sometimes quit work an hour or so before the end of his work day and a dollar or more short of his nine dollar quota. Although his second shift was scheduled to work from 3 P.M. to 11 P.M., Tony oc-

<sup>1</sup> Donald Roy, "Quota Restriction and Goldbricking in a Machine Shop," *American Journal of Sociology*, LVII (March, 1952), pp. 427-442.

<sup>2</sup> *Ibid.*



asionally left the shop at 7 o'clock to lose a half shift's pay on a quota-yielding job.

Gus Schmidt, another speed-drill operator occasionally quit work short of quota earnings.

Gus quit work at 9:45 tonight, after he made 7.95 dollars.

"To hell with it," he said. "Let the day man run 'em. He likes 'em. He turned in 9 dollars today."

"You've got time to make another dollar yourself," I said.

"To hell with that job!" Gus exclaimed. "I'm not going to bust my neck any more on it! Let the day man run it!"

Dooley complained bitterly about the flow of poor piecework jobs to his radial drill. Yet, according to the record, Dooley laid off work to avoid a good piecework job that would yield him his 10 dollars per day quota as radial man.

Dooley returned to work tonight, and was disgusted when he found that Jack (day man on Dooley's machine) had not finished the frames. He laid off yesterday so Jack would finish them up.

Furman Hanks, second shift operator of a four-spindle gang drill and breadwinner for a family of ten, led the shop in complaining about the receipt of poor paying piecework jobs. Yet Hanks would occasionally take a day off to avoid working on a non-piecework operation paying an 85 cents an hour day rate. Since his day shift partner, Ralph Leeds, expressed in identical fashion this disinclination to work for day rate, it would seem that the two men were involved in a mere balance of pay losses. It was noted in Hanks' case, at least, that one day's pay loss could lead to another.

Three of the drill men, including Hanks, laid off today. According to the multiple drill man, Hanks mentioned yesterday that since Saturday would not be a time and a half day for him, he was not going to show up. (He laid off Monday, thus losing his time and a half for Saturday.) He said he wasn't going to work for straight day rate on Saturday.

Al McCann, radial drill operator, kept the writer informed of his chronic financial distress and consequent pressing need for

good piecework jobs, yet McCann chose to lose three days' wages during a general layoff for inventory rather than work at day rate counting stock in the salvage department. Also, on several occasions McCann confided his lack of interest in high pay: "Oh, I don't want the high pay anyhow. . . . It doesn't do any good to get high pay. I'm not a bit better off."

It is not claimed here that the observations presented above indicate a lack of economic rationality in the operators. In fact, it is possible that some sort of careful hedonistic calculation was involved in many of the cases cited. The insistence is merely that further probing of worker response to piecework is needed.

In this search for the basis of response to the piecework incentive—and it should be kept in mind that the machine operators did put forth vigorous effort, with few exceptions, when quota earnings were felt attainable, but only then—the writer's own introspections, as recorded in his work diary, might prove helpful.

In the beginning the writer was slightly contemptuous of the shop's incentive system, considering it neither mature in its appeal as a motivational device nor honest in its claims to be a method of elevating the wage level of industrial workers. Though his sympathies were with the operators, and he observed their codes and procedures to the best of his ability, either through that sympathy or to avoid censure from his fellows, he failed to understand their keen interest in achieving quota production and their intense concern over piecework prices. He laid his own indifference to quota attainment, or "making out," to the fact that machine operation was not his permanent work, not knowing at the time that the others, for the most part, did not consider it their permanent work either.<sup>3</sup>

The writer's attitude changed from mere indifference to the piecework incentive to a determination not to be forced to respond, when failure to get a price increase on one of the lowest paying operations of his job repertoire convinced him that the company was unfair. Light scorn for the incentive system turned to bitterness.

<sup>3</sup> Roy, "Restriction of Output in a Piecework Machine Shop," *op. cit.*, pp. 490-498.

Several months later, however, after fellow operator McCann had instructed him in the "angles on making out," the writer was finding values in the piecework system other than economic ones. He struggled to attain quota "for the hell of it," because it was a "little game" and "keeps me from being bored." He felt that he was the only one motivated by such considerations, thinking that the other operators were being induced to work for higher pay in the manner of the horse led to pull the wagon by a carrot hanging in front of his nose.

In addition to escaping the monotony of factory labor by "playing a game," the writer found that there were physiological advantages in speeding up. He found that fast rhythmical work seemed less fatiguing, although the reduction of fatigue may have been closely related to the reduction of boredom. He discovered further that the same job that had bored and wearied him as a "time study," or non-piecework operation, now interested him and gave him exhilaration on piecework.

The writer made occasional diary comments on such values of piecework for the remainder of his stay in the shop. Recorded comparisons between piecework and day work experience keep telling the same story.

Dooley watched me set up for the frames, and remarked, "You can make out on that if you want to break your neck."

This "breaking my neck" was a welcome relief to the monotony of time study on the replacers. I was so sleepy I could hardly keep awake before I started on the frames, but at eleven o'clock I felt bright and wide awake. This set me to thinking in a new light about the value of the piecework system.

My legs were very tired tonight. Slow jobs like this one seem to wear me out far more than the fast ones. I mentioned this to Johnny.

He said, "That's the way with me. I've got to keep my mind occupied or I get bored, and it wears me out. I can't stand around either."

When I am going hell bent for election on a good piecework job, the evening passes very swiftly and I do not realize that I am tired until it is all over. On these day work jobs I get so bored I could stand in the aisle and yell; and my knees feel tired from standing in one spot so long."

During the course of his employment in the shop the writer discovered that other operators felt the same way, in fundamental respects, about piecework. Mike Koszyk, radial drill man, was one who found that time sped along on piecework and dragged on day work.

Mike compared day work with piecework the other day.

"On piecework I hear that time clock behind me go 'click, 'click, 'clicking off the minutes. But on day work I know it's no use looking around to see what time it is, because the hands won't have moved any."

Al McCann was another who brightened up on good piecework operations.

McCann was given an order of the car replacers tonight. When he received the order, he was disgusted, saying, "Guess I'll be staying home tomorrow."

But after working an hour on the replacers McCann suddenly noted, "I'm making money!" He had done thirteen an hour, at ten cents apiece. Immediately he brightened up, and went at his work with vigor.

McCann expressed his feelings toward his work in terms of making money. It will be recalled, however, that he was the operator who passed up an opportunity to gain three days pay during inventory. Also, he insisted on other occasions that he didn't "care about the money."

Could the process of striving for and achieving such a goal as quota production carry its own reward? Could continual reference to making money represent merely the use of symbols of accomplishment drawn from a "sacrificial view of work"<sup>4</sup> deeply rooted in our economic ideology? Has such symbolization so permeated shop communication that machine operators find it difficult to explain in other terminology their true feelings about quota attainment?

Perhaps we might find the key to piecework incentive in Dewey's distinction between "having an experience" and mere "experiencing things."<sup>5</sup> The difference between a quota-yielding piecework job and day work, in operator experience, might be

<sup>4</sup> Morris Rosenberg, "The Social Roots of Formalism," *Journal of Social Issues*, V (Winter, 1949), p. 21.

<sup>5</sup> John Dewey, *Art as Experience*. New York: Minton, Balch and Company, 1934, pp. 35-57.

the difference between activity that purposefully utilizes resources and overcomes obstacles in moving toward an anticipated consummation, and activity that constitutes mere happenings, with but temporal or mechanical connection between them—a succession of events beginning at no particular place and ending at no particular place. In other words, between experience characterized by intention, organization, and completion—a self-imposed and finished task, problem, or game—and experience that is aimless, unintegrated, and concluded with mere cessation of activity.

It would seem that the attainment of quota marked the successful completion of a task or solution to a problem in which the outcome was largely controllable by the operator, although chance factors were also important determinants of results. Making quota called for the exercise of skill and stamina, and it offered opportunity for self-expression. The element of uncertainty of outcome provided by ever-present possibilities of bad luck made quota attainment an exciting game played against the clock on the wall, a game in which the elements of control provided by the application of knowledge, ingenuity, and speed, heightened interest and lent to exhilarating feelings of accomplishment. Although operators constantly shared their piecework experience as a chief item of conversation, and always in terms of making money or not making money, they could, in reality, have been communicating game scores rather than financial successes or disappointments. It is doubtful if any quota-attaining operator ever believed that he had been making money in the sense of improving appreciably his financial status. Had anyone been able to communicate accurately such a conviction, he would have been laughed out of the shop.

It could be said that playing the make-out game had its negative as well as its positive values. It broke the monotony of repetitive work and made the long day pass. Although on day work the operator had only the pause at lunchtime to break up the meaningless flow of time, he had in his piecework game an hour-by-hour series of completions that served to mark his position in relation to the larger completion of the day's work. For the operator

engaged in a good piecework job, time moved in a rapid succession of intervals toward the final hour of quitting the plant.

But operator interest in quota piecework seemed to have its diminishing returns. McCann, for instance, expressed feelings of boredom on one job that was a never-failing source of premium pay; his claim was that he had performed this particular operation so much that he could do the work in his sleep. His experience suggests that making out on piecework could be a stimulating game only as long as the job represented a real challenge to the operator, only as long as the element of uncertainty was present in the activity's outcome.

If making out lost its value as a game when operator control over the job became so complete that winning degenerated into mere routine, it also lost such value if the element of uncertainty became too predominant over the element of control, that is, if bad luck became too frustrating to the application of skill, the job became nerve-racking. For instance, on one operation the work involved such a high rate of tool breakage at unpredictable intervals that quota attainment seemed to be more a matter of luck than of skill. The nervous strain suffered by the writer and other radial drill men who shared this operation made the job assignment a thoroughly unwelcome one.

Even in a setting of authoritarian administration and worker-management conflict the make-out game induced job interest and productive effort. In fact, during the period of his greatest work effort and enjoyment of the employment of skill and energy the writer's hostility toward his supervisors was on the increase, and his antagonism toward the company was at least not decreasing. It should not be surprising that effort on good piecework jobs could accompany hostility toward management even if making out did not provide the pleasure of playing a game, for quota attainment in its connections with inter-group and intra-group relations provided social rewards. For one thing, making out meant a reduction in the rate of interaction with supervisors. When an operator achieved his quota, or showed signs that he was in the process of doing so, he was left alone.



Pressures from foremen and superintendents were applied only when performance fell short of day rate. Furthermore, as soon as an operator produced his quota his time was his own. Making out in six hours meant two hours of freedom—hours which he could, and usually did, spend in pleasant association with his fellows.

But reference to manipulation of the rate and direction of social interaction does not tell the whole story of the significance of make-out freedom in operator-supervisor relations. If he happened to be inclined to express his aggression toward his foreman, the made-out operator could use his freedom toward that end. Instead of shutting off communication, he could initiate action, for a change, by flaunting his freedom in the supervisor's face. The writer derived keen satisfaction during the latter months of his employment, the months of his peak skill, in conspicuous loafing in the presence of supervision, after quota attainment. Since worker inactivity, even after the completion of a fair day's work, seemed to violate a traditional supervisory precept of keeping the appearance of being busy even if there is nothing to do, making out in four or five hours could be used as a way of getting even with foremen for the pressures that they applied at times when quota was unattainable.

When operator relations with the despised time study men are considered, the connection between quota attainment and inter-group conflict stands out quite clearly. In the intensive conflict interaction that characterized relations between machine operators and the Methods Department, making out was victory for the former. The greater the ease in making out, the less time it took to achieve quota, the greater the beating administered the men who set the piece rates. Thus a game could be played not only against time but also against personalized opponents. The operator watched the clock as he expended his skill and energy, but sometimes the otherwise guileless face of the timepiece on the wall took on the crafty features of the time study man. At other times imagery of the opponent would shift to the scowling face of the foreman when the triumphant operator sat with folded arms in conspicuous idleness beside his completed work, waiting

for a restive boss to say something. Of course no such flaunting of success took place in the presence of time study men or members of the administrative hierarchy above the status of shop superintendent. Since both job-timers and top plant officials appeared mainly in daylight hours, making but sporadic appearances at night, only night shift operators dared to celebrate victory openly. The day men had to deny themselves such bold flourishes.

When operator response to piecework is examined for its significance in intra-group relations, it can be said that the make-out game was played before sympathetic and appreciative audiences. One satisfaction provided the operator by successful piecework activity lay in the approval of his fellows, in informal work group prestige. Any operator could gain recognition by making out on work that carried a piecework price that was difficult to beat, or by cutting down appreciably the time required for making out on any job. Such achievement earned approbation from fellow workers whether freely, grudgingly, or perfunctorily given. The operator would be asked during the shift how he was doing, and at the end of the shift it was always "Did you make out?" If no one asked the question, the successful operator could always make an announcement.

On the other hand, failure to make out on jobs that had been established by the work group as quota operations meant loss of prestige, unless such failure could be explained by reference to factors outside the control of the operator or by insistence that the defection was deliberate. Such explanations were always offered.

It might be said that the operator's efforts toward attainment of quota were in conformance to the codes of the work group. It is obvious that the quota itself, as a limit to output, was a product of the group and a function of its relations to managerial groups; intra-group pressures were immediately brought to bear on the excess earner. Not quite so obvious, perhaps, was the relationship between attaining quota and group norms of accomplishment. Failure to observe the informal rule to make out on quota operations did not carry the harsh penalties that went with violation of the rule not to exceed the established maxi-

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mums, but it was easy to observe that operators felt restive, or guilty, when they failed to make out on quota jobs. Although such emotional disturbances may have been the consequences of frustration that had but minimal social reference, it was made clear to the writer that not attempting to make out on good jobs was not one of the many approved means of aggression against management.

To estimate the relative influence of the various reward elements in piecework incentive would be difficult, and no guesses concerning which is primary and which is secondary or supplemental will be hazarded. All that can be said here, in regard to the particular situation studied, is that satisfactions in producing at quota levels of output included those of (1) playing a game, (2) enjoying free time, which meant in turn a degree of power to control interactional channels, (3) expressing aggression against managerial groups and specific members of management, and (4) receiving approbation or avoiding disapprobation from members of the in-group.

These various satisfactions were inter-related and mutually reinforcing. For example, regardless of where the major emphasis should be put, the bearing of group norms on the game playing is fairly clear. Operatives were under intra-group pressures to play the make-out game, and to play it according to the rules. It will be noted that operators did not try to earn a dollar and a half per hour, nor did they aim as low as the base pay rate of 85 cents an hour. Aiming too high would have brought extreme criticism and possibly severe penalties from the group, and aiming too low would have engendered scorn. Concrete cases recorded in the writer's work diary indicate the actuality of each of these suppositions. The question might be asked: If playing a game carries its own reward, why could not the operators have vanquished the boredom of day work operations by setting production goals in conformance with the nature of the work and their own capabilities? The answer, in terms of group codes, would be that anything greater than very modest output on day work operations might jeopardize chances for favorable piecework prices if and when such operations were eventually studied and timed by

the Methods Department. So the operator must play the make-out game and play it for group-set quota stakes. Another answer might be provided by pointing to the difference between playing marbles for fun and playing for keeps.

It might be claimed, in an analysis of piecework incentive, that the quota striver is also acting in response to the wishes of a supervisory group, for the foremen of the shop not only wanted operatives to make out, they exerted pressures on their men to do so. Some coincidence of the demands of the two groups is indeed recognizable—conflict did not prevail in all phases of inter-group activity. But adherence of the operatives to work group norms and rejection of supervisory standards is made abundantly clear when selection of jobs for the field of play and the establishment of specific goal levels is considered. Foremen exerted pressures for make-out performance on nearly all piecework jobs, and they were satisfied with 85 cents per hour, or base pay rate achievements. Supervisory pressures were rarely effective when they conflicted with patterns maintained by the work group.

Said Steve (superintendent) sharply, a couple of minutes after the 7:15 whistle blew, "I want 25 or 30 of those by 11 o'clock!"

I smiled at him agreeably.

"I mean it!" said Steve, half smiling himself, as McCann and Smith, who were standing near us, laughed aloud.

Steve had to grin in spite of himself, and walked away.

"What he wants and what he is going to get are two different things," said McCann.

The penalties for non-response to supervisory production demands were reserved for those operators whose performance fell appreciably below work group make-out standards. In such cases fellow worker contempt usually accompanied managerial sanctions.

The observations presented in this paper do not indicate that economic motivation played no part in the make-out efforts of the machine operators of the shop investigated. These materials merely suggest that piecework may have provided a complex of incentives within which the economic may indeed have been present. Piecework incentive may include, but seems not to be

synonymous with, economic incentive. If the piecework system succeeds, that is, stimulates operatives to put forth great productive effort, it does not necessarily follow that this stimulation may be attributable to economic incentive. Nor would the failure of piecework provide evidence of the inoperability of economic incentive. It could be that such worker expressions as "I don't care about the money" indicate not the inadequacy of economic incentive per se, but an absence in the situation of possibilities of economic reward of sufficient magnitude to stimulate caring. It would seem that economic incentive would be heavily involved in the activities of those who participated in the California gold rush of 1849. At what level of earning opportunity a recognizable enthusiasm for making money would appear on the factory production line is not known.

Nor is it suggested that, because the various piecework rewards listed here interlock with and reinforce each other, there is a necessary connection between them that must hold in all piecework situations. Certainly other combinations are possible. Obviously the make-out game incentives are

not tied inextricably to those arising from intergroup conflict. The observations of James F. Lincoln<sup>6</sup> indicate that vigorous worker effort to attain piecework production goals can be found in situational contexts of labor-management harmony, though to what extent piecework interest in the Lincoln plant is centered on game scores and to what extent the incentive is more pay is open to question. It would be logical to assume that the most efficient production situation would be one in which the more immediate short run goals of the make-out game would be linked to the longer range goals of the game of business enterprise shared by workers and managers as fellow participants.<sup>7</sup> In such co-operative organization the scattered bits of "having an experience," which in the usual piecework system remain unlinked to each other, would be joined by wider systems of meaning within a total work life that makes sense.

<sup>6</sup> James F. Lincoln, *Lincoln's Incentive System*, New York: McGraw-Hill, 1946.

<sup>7</sup> Russell Davenport, "Enterprise for Everyman," *Fortune* (January, 1950).

## SCALING INTERVIEW DATA<sup>1</sup>

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It is very rare indeed to find that the application of the Guttman scalogram technique reproduces every individual scale score with perfect accuracy.<sup>2</sup> As a

<sup>1</sup> A more detailed record of the study reported here is available in the writer's unpublished master's thesis. See *An Experiment in Scaling Interview Data*, Stanford University, June, 1951. The writer is indebted to Professor Paul Wallin, Stanford University, who directed this research.

<sup>2</sup> See Stouffer, S. A., et al., *Measurement and Prediction*, Princeton University Press, 1950, for a bibliography of literature concerning the theory and methods of scaling. The least technical exposition of the theory is to be found in E. A. Suchman, "The Logic of Scale Construction," in *Educational and Psychological Measurements*, 10 (Spring, 1950), pp. 79-93. The literature on the subject is readily accessible so the writer will assume a knowledge of scaling theory on the part of the reader.

matter of fact, research workers are frequently faced with results in which the utility of a scale is in question simply because of a few errors in reproducibility.<sup>3</sup> Any means, therefore, which might contribute to increasing reproducibility is a matter of importance to those desiring to use this technique.

Until now the scalogram technique has been used primarily on questionnaire data. It has been asserted frequently that the interview is superior in terms of the accuracy of its data when compared with the questionnaire; however, there is less than complete consensus on this point.<sup>4</sup> There-

<sup>3</sup> See Stouffer et al., *op. cit.*, especially chapters 3 and 5.

<sup>4</sup> For more detailed discussion, see Paul L. Lazarsfeld, "The Controversy Over Detailed Inter-



fore, an important question is posed as to whether the interview technique provides a method of securing data which, in terms of Guttman scale analysis, is superior to the questionnaire method.

It could be argued that some, if not all, of the error<sup>5</sup> in scale analysis of questionnaire data, results from possible conditions in the questionnaire situation such as uninterested participation or faulty reading of the questions by the respondent. Further, it could be argued that these weaknesses in the questionnaire method are comparatively absent in an interview because of the interviewer's control of the situation. To the extent that the above assumptions are valid, it might be expected that an interview-based scale would produce a higher coefficient of reproducibility than would a comparable questionnaire-based scale. Similarly it might be expected that an interview-based scale would yield a closer approximation to the theoretically perfect intensity component.<sup>6</sup>

The major restriction on these expectations is the fact that interview data which are not categorized by means of predetermined response options must be rated by judges, an operation not required by the questionnaire method. The ratings of interview material might introduce a degree of error equal to or greater than that occasioned by the uncontrolled conditions of the questionnaire situation.

In an attempt to study this problem empirically a comparison was made of nine rank orderings<sup>7</sup> of 100 subjects<sup>8</sup> on the

favorability of their attitude toward marriage. These subjects were *not* randomly selected from any known population because the purpose of the research was to contrast two methods of securing data rather than to make inferences about human populations. When the research was originally designed no tests of significance were planned since the only anticipated difference, between coefficients of reproducibility, is not amenable to such tests when based on the Guttman technique of scaling. For these reasons, measures of representativeness and randomness were not sought.

The nine rank orderings were based on scale scores derived from two sets of data gathered from each subject. The first set consisted of responses to questionnaire items having predetermined response options. The second set consisted of rated interview data obtained from them.

These data were obtained as follows: the 100 subjects each filled out a questionnaire containing thirteen items judged to pertain to favorability of attitude toward marriage.<sup>9</sup> For purposes of studying the intensity component, each of the thirteen items was followed by a question relating to the intensity with which the subject held the attitude indicated by the item, e.g., "How certain are you of this opinion?"

An interview appointment was made with each subject. Half the subjects were interviewed approximately one week before the questionnaire was filled in and the other half one week after answering the questionnaire. This was done in order to determine to what extent, if any, exposure to one measuring device affected the results of later exposure to the other device.<sup>10</sup>

The interviews were recorded on a Soundsciber. The interview included the same thirteen questions which made up the questionnaire. After the first response to

views—An Offer for Negotiation," *Public Opinion Quarterly*, 8 (1944), pp. 38-60; and Paul Wallin, "The Prediction of Individual Behavior from Case Studies," in Paul Horst *et al.*, *The Prediction of Personal Adjustment*, Social Science Research Council Bulletin, No. 48, 1941, pp. 181-239.

<sup>5</sup> Exclusive of non-scale error or that which is a consequence of a quasi-scale.

<sup>6</sup> Stouffer *et al.*, *op. cit.*, chapter 7, especially pp. 216-218.

<sup>7</sup> All obtained by utilizing the Cornell technique of scale analysis. See L. Guttman, "The Cornell Technique of Scale Analysis," in C. W. Churchman *et al.*, *Measurement of Consumer Interest*, University of Pennsylvania Press, 1947, pp. 60-84.

<sup>8</sup> The subjects all were unmarried, white, male university students. Their mean age was 19.1 with a standard deviation of 1.16. Sixty-four per cent of the sample was Protestant; fifteen per cent Catholic; five per cent Jewish. Sixteen per cent claimed no religious affiliation. The mean annual

income of the subjects' parents was \$14,300 before taxes in 1950.

<sup>9</sup> This series of items was constructed, tested for scalability and found to have a coefficient of reproducibility of .93 for a sample of 100 unmarried male university students in a separate study. See *Attitude Toward Marriage*, unpublished Master's thesis by R. J. Hill, Stanford University, 1951.

<sup>10</sup> There appeared to be no significant differences between groups resulting from whether the respondents filled out the questionnaire first or completed the interview first.

the question, the interviewer asked further questions in an attempt to ascertain what the response "meant" relative to the interviewee's other responses and relative to the responses of other interviewees. After a final response was obtained (which often differed from the initial response), the subject was asked how certain he was of his answer. The "certainty" response to each of the questions provided data for the intensity component of the scale from which the cutting point between "favorable" and "unfavorable" attitudes is obtained. The procedure described was repeated for each of the thirteen questions.

The investigator rated the responses of the subjects to each of the thirteen questions and the associated intensity items. The ratings were made directly from Sound-scriber records. The rating categories applied were the same as the response options for each of the questionnaire items. The interview responses were rated independently by a second judge.

The nine rank orderings constructed from these data were derived from: (1) responses to 13 questionnaire content items; (2) responses to 13 questionnaire intensity items; (3) ratings by the first judge of responses to 13 interview content items; (4) ratings by the first judge of responses to 13 interview intensity items; (5) ratings by the second judge of the same responses to the 13 interview content items; (6) ratings by the second judge of the same responses to the 13 interview intensity items; (7) a combination of ratings by the first judge of interview content responses of 50 subjects and ratings by the second judge of interview content responses of the other 50 subjects (first combination method); (8) a combination of responses of 50 subjects to the 13 questionnaire content items and the responses of the other 50 subjects, rated by the first judge, to the 13 interview content items (second combination method); (9) a combination of approximately 50 per cent of the responses of each respondent to the thirteen questionnaire content items with the responses to the remaining items derived from the interview, selected in such a manner that 50 per cent of the total responses to any given item were from questionnaire and the other

50 per cent were from interview (third combination method.)

These scales were analyzed in terms of the following questions:

1. Can interview responses be rated reliably within the frame of questionnaire response options?
2. Do the interview data yield more perfect indices of unidimensionality than the questionnaire data?
3. Do the interview-based scales result in any different placement of subjects from the questionnaire-based scale placement of the same subjects?
4. Does the collection of data by multiple devices (*i.e.*, partially by questionnaire and partially by interview) have any effect on the coefficient of reproducibility?
5. Does the collection of data by multiple devices have any effect on the rank ordering of subjects?

#### COMPARISON OF JUDGES' RATINGS OF INTERVIEWS

Should differences between the questionnaire-based scale and the interview-based scales obtain with respect either to the criteria of unidimensionality or to the rank ordering of subjects, any one or a combination of three arguments might be advanced: (1) the series of items is unreliable; (2) the interview-response rating process is unreliable; (3) the different methods of obtaining data actually affect the nature of the data.

In the context of this research, item reliability perhaps should be partitioned into two discrete concepts: consistency between items at any given time and consistency of response to each item through time. In the first sense, the criteria of unidimensionality themselves constitute a form of reliability coefficient. Hence, a series of items, such as the present set, which appears to be unidimensional must by definition be internally consistent, or "internally valid" in Guttman's terminology.

Unfortunately no test-retest check of reliability was conducted on either of the two sets of responses alone. It is logically improbable, however, that any set of items which is highly consistent internally should prove markedly inconsistent through a brief span of time. Since the final sets of items did prove to be highly consistent internally,

TABLE 1. COMPARISON OF INTERVIEW RATINGS OF TWO JUDGES

	Per Cent of 1,300 Content Ratings	Per Cent of 1,300 Intensity Ratings
Discrepancies <i>before</i> <i>item combination</i>	1 step 4.5 2 steps 0.38	4.8 0.38
	Per Cent of 900 * Content Ratings	Per Cent of 900 * Intensity Ratings
Discrepancies <i>after</i> <i>item combination</i>	1 step 2.0 2 steps 0.22	3.4 0.10

\* Total ratings are fewer at this point because of the elimination of four items.

should such differences as those described at the beginning of this section be discovered, they should be explained largely in terms of the second or third explanatory option.

In order to obtain an estimate of the reliability of rating interview data in the manner of this research, two judges independently rated all interview responses. Comparison of the two judges' ratings shows a high degree of agreement. In neither their ratings of content nor intensity items was there a discrepancy of more than two steps. Two of the thirteen content items had dichotomous response categories and the remainder had four response categories before

combining. Eight of the intensity items had four response categories and five had five categories before combining.

Table 1 shows the proportion of the 1,300 content and 1,300 intensity responses on which the judges disagreed by one or more steps. The correspondence between the initial ratings is shown along with the correspondence after combining response categories. The total number of ratings per judge after the elimination of four questions was 900 for the content and 900 for the intensity items.

A second estimate of rater reliability was obtained through correlating the rank ordering of one judge's scale ratings with the comparable rank ordering of the other judge. Table 3 below indicates that the coefficient of rank correlation in this case was .97, from which it can be concluded that scale scores for each subject obtained from the independent ratings of the two judges are highly consistent.

A third index of rater reliability in this research consisted of an attempt to scale a 50 per cent sample of the total responses rated by one judge together with the other 50 per cent of total responses rated by the second judge. This was called the first combination method. As is indicated in Table 2, this attempt yielded a coefficient of reproducibility only slightly lower than those yielded by either set of data alone. The rank ordering derived from this first combination method correlated at .94 and .96 with the scaled ratings of the first judge

TABLE 2. COMPARISON OF INTERVIEW-BASED CONTENT SCALES WITH QUESTIONNAIRE-BASED SCALE AND COMBINATION SCALES

	Question- naire Based Scale	Interview-Based Scales			Combination Scales: from Questionnaire and Interview Data	
		1st Judge	2nd Judge	1st Combination Method	2nd Method	3rd Method
Coefficient of reproducibility	0.933	0.952	0.955	0.949	0.886	0.894
Number of items in scale	9	9	9	9	10	8
Number of response options after combination	22	20	20	20	27	18
Average of item modals (average minimum reproducibility)	0.803	0.823	0.827	0.823	0.718	0.683
Improvement over average reproducibilities	0.130	0.129	0.128	0.126	0.168	0.211



and the second judge respectively (see Table 3).

In part the high degree of correspondence between the two judges may be attributed to two factors: (1) the overall homogeneity of the responses to any given item, and (2) the fact that the ratings were made on the basis of generally brief responses to specific questions. However, the fact remains that extremely high correspondence was obtained; should differences either in the criteria of unidimensionality or in the rank ordering of subjects appear between questionnaire and interview data, this fact substantially reduces the possibility that such differences could be attributed to distorted interview ratings.

#### SCALE ANALYSIS OF CONTENT DATA FROM QUESTIONNAIRE AND INTERVIEW<sup>11</sup>

Table 2 presents certain comparisons of the scale obtained from questionnaire responses with the three scales obtained from interview data and with the two scales derived by combining interview and questionnaire data in two ways (second and third combination methods).

In evaluating scales as evidence of unidimensionality, the basic criteria are: (1) the coefficient of reproducibility; (2) the number of items used; (3) the marginal frequencies.<sup>12</sup> Each of these criteria is discussed below in relation to the questionnaire-based scale and the interview-based scales. Discussion of the second and third combination scales is reserved for a later section.

*Coefficient of reproducibility.* In every case, the interview-based scales yielded higher coefficients of reproducibility than did the questionnaire-based scale; the range of superiority was from 0.016 to 0.022.

<sup>11</sup> The remainder of this paper will be concerned exclusively with questionnaire and interview content data. Questionnaire intensity data were scaled as were interview intensity data derived from the ratings of one judge. Both sets yielded quasi-scale patterns as is frequently the case with intensity items. Both sets were plotted, resulting in identical zero points of intensity (at the fifth percentile). The curve of interview data, however, was considerably smoother than was the curve of questionnaire data.

<sup>12</sup> Marginal in the sense that the frequencies are located in the lower margin of the scalogram.

Unfortunately for our purposes, no method has been developed for testing the significance of difference between coefficients of reproducibility based on Guttman scale analysis. Consequently, it can only be said that, in the case of this attitude, universe, and for these subjects, scaled interview data and scaled questionnaire data both exceed the arbitrary lower limit for an acceptable coefficient of reproducibility (0.90). Further, it can be said that scaled interview data yield a coefficient of reproducibility as high or slightly higher than scaled questionnaire data.

*Number of items in the scales.* The number of items used in the questionnaire scale and in each of the interview scales was nine. Item twelve<sup>13</sup> scaled for the questionnaire analysis but not for any of the interview analyses. Item eleven<sup>14</sup> scaled for each of the interview analyses but not for the questionnaire analysis. Items four,<sup>15</sup> nine,<sup>16</sup> and thirteen<sup>17</sup> were eliminated from all scales considered here.

*Marginal frequencies.* The reproducibility of any given item cannot be lower than the proportion of responses found in the modal categories for that item. The minimum reproducibility of a series of items, regardless of whether it constituted a scale, would then be the average of the number of responses in the modal categories of all items used.

The minimum reproducibility for interview-based scales and for questionnaire-based scales was about 0.13 points below the obtained coefficient of reproducibility in every case. The marginal frequencies of these scales were more extreme than is desirable: the closer the average frequency of modal response cells approaches fifty per cent of total frequencies, the more meaningful is a high coefficient of reproducibility. The coefficients of reproducibility of the interview data yielded improvements

<sup>13</sup> "Do you ever have doubts as to whether you will enjoy being a parent?"

<sup>14</sup> "Do you think that you will find (or have found) a person who is a suitable marriage partner for you?"

<sup>15</sup> "How happy do you think you will be if you remain single?"

<sup>16</sup> "In your opinion to what extent will the responsibilities of married life be enjoyable to you?"

<sup>17</sup> "Do you ever have doubts as to whether you will enjoy the sexual part of married life?"

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<sup>18</sup> *Ibid*

over their item modals of from 0.126 to 0.129. The questionnaire data yielded an improvement of 0.130. Although there is evidently no method of testing significance of differential improvement, it is highly improbable that differences of from 0.001 to 0.004 would prove to be significant.

The number of response options after combination is an important consideration, at least when only a very few items are used. While the questionnaire-based scale yielded two post-combination options more than each of the interview-based scales, this fact is not deemed particularly important since each of these scales included nine items.

tained correlations between each of them and any other variable must be identical. If they are different, then one must correlate more highly than the other with any external variable (except in the case where both yield no correlation with another variable). From this it follows that summary measures, as in Table 2, are inadequate for an evaluation of the comparability of two scales. This poses the question whether the two sets of scales order the subjects of the study in an identical or a differing sequence. An answer can be found through correlating scale positions of each subject on the two scales.

Since the purpose of the Guttman tech-

TABLE 3. RANK CORRELATION BETWEEN INTERVIEW-BASED AND QUESTIONNAIRE-BASED SCALES

	Questionnaire Based Scale	Interview-Based Scales			Combination Scales: from Questionnaire and Interview Data	
		1st Judge	2nd Judge	1st Combination Method	2nd Method	3rd Method
Questionnaire-based scale	...	...	...	...	...	...
1st judge	.37	...	...	...	...	...
2nd judge	.34	.97	...	...	...	...
1st combination method	.35	.94	.96	...	...	...
2nd combination method	.64	.52	.54	.54	...	...
3rd combination method	.69	.49	.47	.46	.66	...

From these criteria, an answer can be obtained to the second general question: for this sample of items and for this group of respondents, interview data yield indices of unidimensionality which are quite similar to those derived from questionnaire data; neither source appears to be markedly superior to the other in this respect.

#### CORRELATION OF CONTENT SCORES

It has been shown that the two methods of collecting data can result in almost identical summary measures from which "internal validity" is evaluated. Guttman maintains that "external validity" is in no way determined by internal validity and can be evaluated only by relating the ordered sample to some external variable. If two rank orderings are identical, then ob-

nique is to rank people with reference to a given variable, it was felt that some form of rank difference correlation would be a satisfactory measure of association. Kendall<sup>19</sup> has developed a form of rank correlation which is corrected for ties in both ranks which he has called  $\tau_b$ .<sup>20</sup> This measure, the limits of which are plus or minus 1.00, was used to estimate the similarity of rank orderings between the various scales. The results are presented in Table 3.

<sup>19</sup> Maurice G. Kendall, *Rank Correlation Methods*, London: Charles Griffin & Co. Limited, 1948.

<sup>20</sup>  $\tau_b =$

$$\frac{S}{\sqrt{[\frac{1}{2}n(n-1) - \frac{1}{2}\sum t(t-1)] [\frac{1}{2}n(n-1) - \frac{1}{2}\sum u(u-1)]}}$$

where:

n=number of observations

t=number of ties in one rank

u=number of ties in the other rank

s=sum of positive scores minus sum of negative scores

<sup>18</sup> *Ibid.*, pp. 57-59.

When interpreting this table, it must be remembered that the data being correlated were the responses of the same subjects to the same questions. The only evident basis of differentiation (beyond a brief difference in time of response) is the method by which the responses were collected. In this light, the table is remarkable for the consistently low order of association between questionnaire-based and interview-based rank orderings; the questionnaire-based scale correlates between .34 and .37 with the three interview-based scales.

It is impossible to determine what proportion of unexplained variance is due to unreliability of the items through time; however, since the items are so highly consistent internally and since the time span was brief, we might suspect that far less than the entire amount is attributable to unreliability. We may tentatively conclude then that the method of gathering data may materially affect the rank ordering derived from Guttman scale analysis, at least for items comparable to those used in this research.

#### SCALE ANALYSIS AND CORRELATION OF COMBINED DATA

It has been concluded that the method of obtaining data, in cases similar to the present, may materially affect the outcome of a survey, that a questionnaire survey may order a sample of subjects with marked differences from the ranking based on an interview survey of a comparable sample of the same population. This conclusion poses yet another question: how might a survey, simultaneously utilizing both questionnaire and interview, order subjects relative to the possibly differing orderings based on questionnaire or interview separately? In order to obtain a preliminary empirical answer to this question, the two basic sets of data (questionnaire and interview) were combined in two ways.

The first such combination involved randomly drawing 50 per cent of the 100 subjects and utilizing only their interview responses. All of the questionnaire responses of the remaining 50 respondents were combined with these data. This was called the second combination method. As is indicated in Table 2, this series failed to yield a coefficient of reproducibility above the arbi-

trary lower limit of acceptability; but it failed by only .014 points. The nine items in this set were identical with those of the questionnaire scale. Its improvement over minimum reproducibility is somewhat greater than those of questionnaire-based or interview-based scales. In this sense, at least, this set of items is superior. Scale scores from this set correlated between .52 and .54 with the scores from the three interview scales. This set correlated somewhat higher, .64, with the questionnaire scale.

Another combination scale was constructed as follows: interview responses to all odd-numbered questions were collected from 50 per cent of the 100 subjects. Responses of these 50 subjects to even-numbered questions were taken from the questionnaire. This procedure was reversed for the other 50 subjects: questionnaire responses were taken for each odd-numbered question and interview responses for each even-numbered question. This was called the third combination method.

This set also failed to yield an acceptable coefficient of reproducibility; it failed, however, by only .006 points, hardly enough to reject it as being definitely multi-dimensional. It yielded greater improvement over minimum reproducibility than did any of the other five scales. Items one<sup>21</sup> and five,<sup>22</sup> which scaled in all other sets, were eliminated here. Item nine,<sup>23</sup> which scaled in none of the other sets, scaled in this instance. Item eleven,<sup>24</sup> which scaled only for the interview sets, was eliminated from this scale. Item twelve,<sup>25</sup> which scaled for the questionnaire but for none of the interview sets, scaled here. The remaining items were the same for all scales. This combination method correlated higher with the questionnaire-based scale, and lower with the interview-based scales, than did the second combination method. Both combination methods

<sup>21</sup> "Do you think it would be advisable for you always to remain single?"

<sup>22</sup> "How happy do you think you will be if you marry?"

<sup>23</sup> "In your opinion, to what extent will the responsibilities of married life be enjoyable to you?"

<sup>24</sup> "Do you think that you will find (or have found) a person who is a suitable marriage partner for you?"

<sup>25</sup> "Do you ever have doubts as to whether you will enjoy being a parent?"



yielded rank orderings which correlated higher with the questionnaire than with the interview-based rank orderings.

In tentative answer to the final two general questions, it may be said that, in some instances, the use of multiple collection devices does indeed affect the coefficient of reproducibility and the other indices of unidimensionality. Further, it may be said that the use of multiple collection devices for gathering data may render resulting ranks orderings different from those which would have resulted from the use of one collection device alone.

#### SUMMARY AND CONCLUSION

The research reported here was concerned with the problem of attempting to scale interview responses to questions bearing on the favorability of attitude toward marriage and comparing the results with scaled responses to the same questions obtained from questionnaires. The responses were collected from 100 unmarried, white, male university students. A questionnaire containing thirteen content and thirteen intensity questions concerning favorability of attitude toward marriage was administered to the subjects who also participated in an interview in which the same content and intensity questions were asked. The recorded interview responses were rated and then scaled in the same way as the questionnaire responses.

Three indices of rater reliability were constructed. An examination of these led to the conclusion that open-ended interview responses can be rated quite reliably within

the frame of the usual Guttman questionnaire response options.

It was found that the questionnaire data and the rated interview data both yielded generally acceptable criteria of unidimensionality. Neither method proved markedly superior to the other in this respect.

When rank orderings based on scaled interview data were correlated with similar rank orderings from questionnaire data, however, it was found that the two methods resulted in drastically different placement of the same subjects with respect to the same variable.

Two combination scales, each based in part on questionnaire responses and in part on interview responses, were constructed, tested for scalability and found to yield coefficients of reproducibility lower than those obtained from scales based on one or the other collection device alone. They fell slightly below the arbitrary lower limit of acceptability. These two sets resulted in rank orderings considerably different from those produced either by the questionnaire-based scale or by the interview-based scales, but both combination scales were more similar to the former than to the latter.

Since it was found that each of these three scale sets, interview-based, questionnaire-based, and multi-based, yielded considerably different rank orderings of the same subjects, a question of considerable importance is posed: which of these rank orderings would correlate most highly with relevant external variables? This, however, is a question to be answered only by further research.

## A PRELIMINARY LABORATORY STUDY OF THE ACTING CROWD

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THIS paper<sup>1</sup> gives the results of limited efforts over a period of three years to test a set of hypotheses about the conditions productive of what Blumer<sup>2</sup> has called the "acting" crowd. Although the number of groups studied is small, the findings are presented now in the belief that they seem promising and in the hope that others will be interested in moving ahead with similar projects.

Those of us who teach courses in collective behavior know that its materials are at once provocative and confusing. Among the varieties of collective behavior, crowd action has the added property of being fugitive. Its coming is hard to predict. Once arrived, it is hard to observe. (Imagine trying to interview in a mob!) Within the ethical code of scientists in a free society, we cannot try to produce large-scale crowds. It is not surprising that Strauss<sup>3</sup> was able to find only a dozen or so articles on collective behavior in his ten-year survey of *The American Journal of Sociology*, or that none of these articles involve empirical studies of crowds.<sup>4</sup>

<sup>1</sup>The author developed the basic theoretical scheme and experimental design while a Research Associate in the Research Center for Group Dynamics. Acknowledgments are due to Ronald Lippitt, Ian Ross, and Andrew Kapos for valuable design suggestions and to the several students in a course in collective behavior who acted as volunteer observers for these preliminary runs.

<sup>2</sup>Herbert Blumer, "Collective Behavior," in Alfred M. Lee (ed.), *New Outline of the Principles of Sociology*, New York: Barnes and Noble, Inc., 1946, pp. 165-222.

<sup>3</sup>Anselm L. Strauss, "Research in Collective Behavior: Neglect and Need," *American Sociological Review*, 12 (June, 1947), pp. 352-354.

<sup>4</sup>One attempt to produce crowd behavior is reported by Norman C. Meier, G. H. Mennenga, and H. J. Stoltz, "An Experimental Approach to the Study of Mob Behavior," *The Journal of Abnormal and Social Psychology*, 36 (July, 1941), pp. 506-524. Experimental studies of crowd-related phenomena appear in Floyd H. Allport, *Social Psychology*, Boston: Houghton Mifflin Co., 1924,

There seems to be another important reason for the neglect suffered by the study of the crowd. Somehow, it is hard to fit into any particular conceptual niche. Its significance is in doubt. Together with the difficult methodological problems, this makes it less likely that anyone will use it as a focus of research.

In another place,<sup>5</sup> the writer has suggested that the behavior of crowds as well as the other phenomena of collective behavior may profitably be conceptualized, along with bureaucracies, families, labor unions, political parties, voluntary organizations, and all other groups, as special forms of the interpersonal patterns of influence that appear as people adapt to one another in the course of mastering the problems set by the environment. It was also suggested<sup>6</sup> that we take seriously the dictum of Thomas, Znaniecki, and a host of others that, in theorizing about the causes of any behavior, individual or collective, we conceive of our independent variables in terms of the environment as experienced by those behaving, and that we assume that their acts are efforts to deal with the world as they perceive it. This has many implications. The only one stressed here is its suggestion that, assuming human biology to be a constant for purposes of theory-building, one may predict variations in the organization of a group from variations in environmental problems as its members experience them.

and Alexander Mintz, "Non-adaptive Group Behavior," *The Journal of Abnormal and Social Psychology*, 46 (April, 1951), pp. 150-158. There is little overlap between the variables used in these sources and those of concern in the present study.

<sup>5</sup>Guy E. Swanson, "Social Change in the Urban Community," in Ronald Freedman and others, *Principles of Sociology*, New York: Henry Holt and Co., 1952, pp. 554-619.

<sup>6</sup>Guy E. Swanson, "The Approach to a General Theory of Action by Parsons and Shils," *American Sociological Review*, 18 (April, 1953), pp. 125-134.

In line with this orientation, the writer has gone over the descriptive and theoretical writing about crowds, trying to isolate the essential phenomena involved and to specify the experienced problems that produce them.<sup>7</sup> This led to a conception of the nature of crowds as a particular form of organization for the collective solution of problems—an organizational form that might appear in populations of any size from two on up. With such a conception, it became meaningful to think of producing crowd behavior in small, experimentally created populations.

The present study focuses on the acting crowd—that form of crowd in which be-

## EXPERIMENTAL DESIGN

The experimental design varies the extent to which subjects are acquainted with a task and the extent to which they are acquainted with each other as co-workers on that task. A simplified version of the scheme appears in Figure 1. It is assumed, in keeping with the bulk of the writing on acting crowds, that groups represented by Cell A will be least like such crowds and that groups represented by Cell D will be most like such crowds. In this preliminary study, only data for Cells A and D were obtained. Groups in Cells B and C are clearly intermediate cases. It would be

FIGURE 1. Experimental Variations  
Have subjects had previous experience  
with this task?

		Yes	No
Have subjects previously worked together on this task?	Yes	A	B
	No	C	D 3n 3 F

havior aims at manipulating the environment external to the selves of the members. Conditions productive of expressive crowds, where the behavior seems directed at manipulating the self-images and norms of the participants, will not be consid-

Like any other group, an acting crowd does not spring into being full grown. It has a natural history. There is a period in which people congregate, a period in which they mill around and begin to organize, and a period in which they act collectively to change their world. These periods flow into each other and are far from irreversible. The fact that they can be distinguished does make it necessary, however, to specify which of them is under consideration when one makes predictions. This study focuses on the second and third periods—the stages of milling and of collective action.

<sup>7</sup> Swanson, "Social Change in the Urban Community," *op. cit.*

worth trying to produce them only if the trends in A and D were promising.

Some experimental variables other than the ones chosen might have been used in this study. For example, a number of the hypotheses about crowd behavior seem to derive from the size of the population concerned. Among these is such a hypothesis as the one predicting greater freedom for the individual to express counter-mores sentiments because he can be anonymous in the mass of the crowd. The experimental variables used were chosen on the grounds that the number of phenomena of crowd behavior derivable from them were larger than the number derivable from other variables.<sup>8</sup>

The groups studied to meet the conditions of Cell D consisted of six populations, each containing three subjects. Three of these

<sup>8</sup> *Ibid.*



populations consisted of male subjects; three of female subjects. All subjects were members of a very large lecture section (over 300 students) in Introductory Zoology at the University of Michigan. All were second-semester Freshmen. They were selected at random from among over 150 members of the class who volunteered as participants in "a study of the way people handle problems." The age for any subject is within one year of the median age for his class in the University. Non-white students and students born outside the United States were eliminated. An item in the questionnaire administered during the experiment validated the judgment that this method of selection would control for degree of previous acquaintance among group members. Questionnaire results also show that all subjects accepted as correct the purpose of the experiment as outlined to them by the experimenter. Checks for inter-group differences in age and grade-point average revealed no differences.

Subjects in each population to be studied were notified by letter of the time and place of the experiment. On arrival, they were asked to wait in the hall outside the experimental room, occupying chairs so arranged that they could not communicate with each other. A few magazines were on hand for them to leaf through while waiting.

When all had arrived, the subjects were taken into the experimental room. This room, about 21 by 24 feet in size, is outlined in Figure 2. Subjects were asked to sit in chairs R, Y, and B. Each was given an armband to wear—one red, one yellow, and one blue. No comment was made on the purpose of these armbands. The experimenter then read these instructions:

As you may know, there is a lot of interest these days in the ability of groups to handle problems. Most previous research has studied the way groups work on discussion-type problems. During the war, and at present, social scientists have been exploring the way groups deal with problems involving action as well as discussion.

The problem today is such an all-around problem. It requires many different skills for its solution. It is patterned on the situations created by the State Department and the Office of Strategic Services for selecting

groups of skilled personnel to handle particularly difficult jobs.

After the session is over, we will be glad to answer your questions about the details of this situation and the reasons for our interest in it.

Once things begin, those of us working on the project will be very busy. You should treat us as part of the furniture, paying as little attention to us as possible.

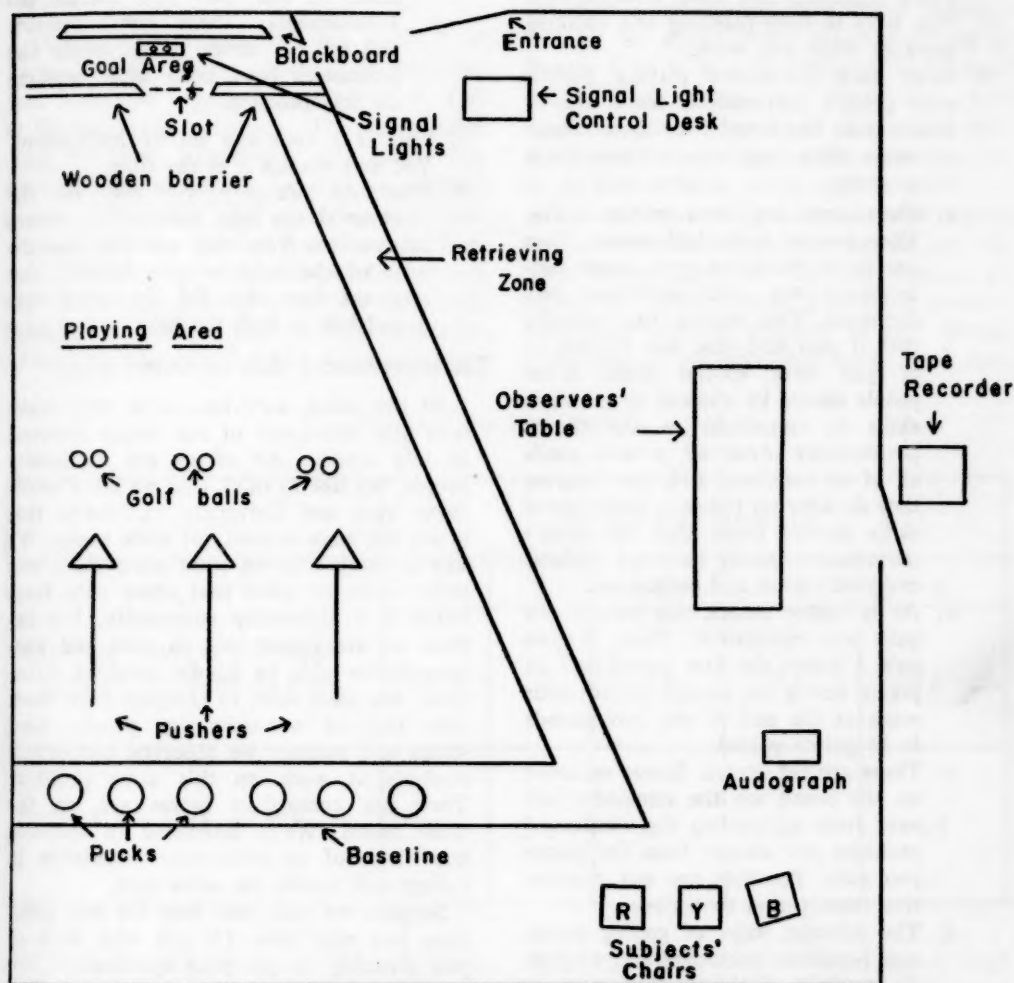
You are free to talk to each other if you wish, and you may talk about anything you please.

At this point, each subject was handed a copy of the following instructions, and all were asked to read them along with the experimenter:

#### PARTICIPANT'S INSTRUCTIONS

Today two other people and yourself are going to solve a problem. The problem is that of using the equipment you see over there to get a high score.

1. We can tell you only a few things about the way to solve this problem. Here are the hints we can give about how to get a high score.
  - a. Points are gained by getting some golf balls in some way through the slot in that wooden barrier over there.
  - b. Points are gained by getting some golf balls in some way to hit against that wooden barrier over there.
  - c. The way to get the most points requires that at least one person shall use one piece of each of these types of equipment simultaneously. (Experimenter points to *pucks*, triangular "shuffleboard" *pushers*, and *golf balls*.)
  - d. The things that you do to gain points are done on this side of this line. The experimenter will demonstrate to show you the line. (Experimenter points to baseline.)
  - e. The way to get the most points is to work together, plan together, and, at all times, to keep moving.
2. You can find out whether the things you are doing are gaining points or losing points by watching the lights over there under the blackboard. The red bulb will flash on every time you do something that is giving you a penalty. The green light will flash on every time you do something that is gaining points for you. (Experimenter flashes lights to demonstrate.)

FIGURE 2. Diagram of Experimental Room  
Hall

Floor dimensions drawn to  $\frac{1}{4}$ " scale

You should do your best to keep those lights flashing. If you do things that get no response from the lights, you are fiddling away time without learning how to gain points and avoid penalties.

Every time a light flashes, it should be a problem for you. You should try to make a joint analysis of what you did that gained points or penalties.

Don't hesitate to do things that will give you penalties. You must gamble a lot and try a lot of things to find out what makes this work—to find out how

you can build up points. College students tend to do too much talking and planning, and too little playing, or *vice versa*. This problem requires that you do both things at once if you are to get as good a score as you can.

3. You will have a limited amount of time in which to play. The plan is that you will have a total of 23.5 minutes to solve this problem. You will play for 3.5 minutes, take a 1.5-minute intermission, play for another 3.5 minutes, take a second 1.5-minute intermission, and so on.

During the intermissions, you will return to your present seats.

4. This means that this is a kind of emergency situation. You have to work fast. You have to keep planning and thinking together while you work.
5. After each 3.5-minute playing period, your group's performance score will be recorded on the board over there. There are some things you should know about these scores.
  - a. The scores are total group scores. They are not individual scores. Your job is to maximize your *team* performance, not your individual performance. This means, for example, that if you find that one or two of you have special skills, those people should be allowed to use their skills to contribute to the team's performance. After all, a team needs all of its members, and, just because they do different things or have special skills doesn't mean that the team's performance could be good without everyone's work and enthusiasm.
  - b. As in many games, the points you gain are cumulative. Thus, if you gain 4 points the first period and 10 points during the second period, your score at the end of the two periods is 14 points gained.
  - c. These are net scores. Scores recorded on the board are the remainder left over from subtracting the number of penalties you receive from the points you gain. Penalties are *not* cumulative from period to period.
  - d. The different ways of getting points and penalties have different weights. An example of this is the set-up in football of giving 6 points for a touchdown, 1 point for making the kick after touchdown, and giving different numbers of yards in penalties for different infractions of the rules.
  - e. At the beginning of play, you will get an immediate credit bonus of some gain points. In other words, you start with some gains "in the bank."
6. Now, for some special notes:
  - a. Please don't throw the equipment. It breaks.
  - b. Please speak up so the observers can hear you.
  - c. Once we get under way, we cannot answer any questions. You will get your instructions from this recording machine. (Here, experimenter points

to the Audograph): It will tell you what to do and how much time you have remaining. Also, from time to time, it will give you special announcements. These announcements will tell you about things people like yourselves have done while working on this problem.

7. This is a complete set of instructions. You may consult it at any time.
8. Note: As you play, you may ask the observer at the light buttons for certain information. You may ask him for the color of the light he just flashed. You may ask him who did the thing that caused him to flash the light.

The experimenter then continued with:

At this point, we'd like to be very frank with you about one of our major interests in this session. All of us are University people. We like to think that we learn something from our University experience that is not the mere contents of some books. We like to think that we learn some skills and some habits of mind that come only from living in a University community. We believe we are better able to plan and analyze—better able to handle involved situations. We shall want to compare your work with that of non-university people. Last spring and summer we observed high-school students at work on this same problem. Their net cumulative scores are on the score board. We're interested in knowing how those of us with some experience in college will handle the same task.

Suppose we take time here for any questions you may have. I'll talk with each of you privately to get your questions.

After talking with each subject, the experimenter continued:

All right. Now that you've seen what the problem is, and before you go to work on it, will you use this questionnaire to tell us about your feelings about the whole situation? Work rapidly, but try to be as accurate as you can.

The completed questionnaires were collected, and the experimenter said:

All right. Listen to the record for instructions.

Finally, when the five playing periods were over, the experimenter broke in with:

O.K. Sit down and catch your breath. We have one more thing we'd like you to



do. Here is another questionnaire. Work rapidly and accurately. When you've finished this, we'll try to explain what this was all about.

Populations represented by Cell A of Figure 1 were made identical in composition with the populations studied in Cell D through randomization. The only difference in their experience was that they were invited to take a ten-minute practice period just after the instructions were given and before the first questionnaire form was distributed. During this practice period, the Audograph played a record reminding them of the elapsing time at half-minute intervals, and the experimenter signalled points

lying within the playing area, and when a subject allowed any golf ball to roll outside the retrieving zone. The net score pattern allegedly achieved by high-school students and by the group under study is shown in Figure 3 as it was recorded on the blackboard. Each group was given the same "scores." The ambiguity of the scoring system made it possible for the experimenter to do this without being challenged.

The only remaining feature of the experimental situation began just as the populations were instructed by the Audograph record to "Begin play. You have twenty-three and one-half minutes to go." Simultaneously, in an adjoining room, and con-

FIGURE 3. Net Score Patterns

GROUPS	PLAYING PERIOD				
	1	2	3	4	5
Average Scores of High School Student Groups	15	37	51	92	103
Scores of This Group	9	39	44	82	105

and penalties with the green and red lights. It is this ten-minute practice period that is intended to supply the difference in acquaintance with the task, and with each other as co-workers on the task, required to differentiate Cells A and D.

During all playing periods, the experimenter flashed a green light whenever a subject, by driving a puck with a pusher, managed to get a golf ball of the color of his armband to hit the barrier or to roll into the goal area. He flashed a red light when a subject shot from any position other than from behind the baseline, when a subject touched golf balls lying within the playing area with anything but pucks propelled by pushers, when a subject, by any means, set in motion golf balls other than those colored like his armband and

ceased by the half-opened door, a tape recorder began to play.<sup>9</sup> From that point until the end of the last playing period, the tape recorder played a program that alternated music and speech at two-minute intervals. The music was chosen to be familiar to college freshmen. The alternately presented readings were chosen to catch the ear. They included a selection from Whyte's<sup>10</sup> article on a slum sex code, part

<sup>9</sup> The Audograph used was a standard Gray model. It was turned up to full volume. The tape recorder, a Pentron, Model 9T-3, was turned to 4 on the volume control and 5 on the tone control. The Audograph and tape recordings were made with the same settings of their controls as those used when they were played.

<sup>10</sup> William F. Whyte, "A Slum Sex Code," *The American Journal of Sociology*, 49 (July, 1943), pp. 24-31.

of a recent Fundamentalist tract on the nature of Heaven, the crescendo lines of *The Communist Manifesto*, a *New York Times*' report of a vegetarian Thanksgiving dinner, and the end of a Detroit Tigers' baseball game, complete with the jingle advertising, "Brewster, the Goebel Beer rooster."

#### A NOTE ON CONTEXT VARIABLES

It should not be thought, of course, that differences in familiarity with the task and with each other as co-workers on the task, will produce the phenomena of the acting crowd under all conditions. Along with the usual negative prescription that such variables as previous acquaintance, group size, sex distribution, and objective degree of success must be controlled by randomization or by selection of subjects and situations, there are certain conditions that must be given positive prescription. Present understanding<sup>11</sup> suggests that a satisfactory experimental situation for producing crowd behavior must include the following features.

1. The problem must be seen as one requiring the members of the population to work together. If this is not the case, no interaction need occur.
2. The nature of the problem must be clear enough to permit some focus for organizing behavior, some criteria for judging whether to follow one line of action or another.
3. It must appear that there is some chance of success in solving the problem. Apathy is predicted if failure is assured in advance.
4. The problem must be one requiring immediate solution. Slow, rational deliberation would be predicted if there is no urgency for solution.

The experimental design was built to give these perceptions to the subjects as they began problem-solving together.

#### FINDINGS: DEPENDENT VARIABLES

Given differences in familiarity with the task, and with each other as members of a population working on this task, and given the context variables used here, it is possible to predict a large share of the phenom-

ena of the acting crowd. Each prediction will be stated together with the bases for deriving it from the independent variables. This will be followed by a report of the methods for gathering data and the findings from the data obtained.

1. *Suggestibility*. It is a universal report in the field studies of acting crowds that their members readily accept problem-relevant suggestions. Writers like Le Bon<sup>12</sup> deplored this uncritical acceptance of ideas, attributing it to the deterioration in individual intellectual functioning that takes place when men work together. More recently, the interpretation has followed that of many experiments on suggestibility. Laboratory studies show that people are more likely to accept others' ideas when they have none of their own, and that this acceptance occurs with increased frequency as people feel increasing pressure to make decisions. This is the situation of the members of acting crowds. They are faced with an urgent problem for which they have no solution. This being true, they grasp at such ideas as are available as a guide for their behavior.)

Two ways of observing suggestibility were included in the experimental design. First, subjects were asked to respond to two questionnaire items on the second questionnaire form:

✓ How often do you remember going along with ideas that came up, during the course of playing or resting, about which you felt skeptical?

— Very often — Often — Occasionally  
— Seldom — Never

How often do you remember ideas coming up during the experiment that you thought should have been given more consideration?

— Very often — Often — Occasionally  
— Seldom — Never

It was expected that members of Cell D groups would be less critical of the ideas that came up during the experiment and would evidence this in their questionnaire responses. There are, however, no differences in the answers given in the A and D groups.

The second observation technique in-

<sup>11</sup> Swanson, "Social Change in the Urban Community," *op. cit.*

<sup>12</sup> Gustave Le Bon, *The Crowd: A Study of the Popular Mind*, London: T. Fisher Unwin, 1903.

volved a direct coding of overt behavior.<sup>13</sup> In each playing period, just after the voice on the Audograph record made his second announcement of the time that remained in that playing period, he followed with something like this: "Attention, please. It is not against the rules to bunch the balls together in the center of the floor." Planted experimentally, these ideas were selected as being likely to seem plausible to the subjects and as neither detracting from, nor improving, the objective performance they would exhibit. An observer noted the number of persons who listened to the announcement, the number of persons who spoke seriously with others about the possibility of carrying out the announced idea, and the number of persons who actually tried to carry out the idea. Group members do not try overtly to carry through on an Audograph suggestion often enough to merit the comparison of the A and D variations on this observation. Listening to the suggestions and discussing them, however, appeared frequently enough to warrant further examination. Analysis shows that D group members are more likely than members of A groups to discuss the suggestions seriously as well as to listen to them. Each group was given a score consisting of the total number of times members displayed either of these two kinds of behavior, and the scores of A and D groups were compared, using the chi-square technique with Smith's correction for small numbers.<sup>14</sup> By this method of analysis, the suggestibility scores of the D-type groups exceed those of the A-type groups at a level of significance beyond .005.

2. *Volume of Communication.* The great volume of communication in acting crowds has been widely observed. It is usually explained as a product of the urgency with which people have to develop a scheme of joint action. Looking upon communication as a set of instrumental acts for manipulating the behavior of others, one would expect that the greater the need for interper-

sonal co-ordination within a given time period, the greater will be the volume of communication.

Observations of communication volume were made by an observer using an Esterline-Angus twenty-pen recording unit.<sup>15</sup> Every time a group member spoke, this observer pressed two buttons. One represented the person speaking; the other, the target of his communication.<sup>16</sup> The observer held the buttons down as long as the subject continued speaking to the same target. Possible targets for a given subject were: either of the other subjects, the rest of the group in general, or, in a few cases, the speaker himself. An inked record was made of this behavior on a paper tape moving at a constant speed. If we define volume of communication as being the number of times any subject spoke to any target during the experimental period, we find that the volume of communication in the Cell D groups exceeds that in the Cell A groups at less than the .03 level of confidence. Parenthetically, there is not a significant difference between the A and D variations in the length of the average communication expressed by their members.

3. *Proportion of Communication Directed Toward the Group.* From the beginning, students of acting crowds observed that people who normally would have no contact with each other, interacted in crowds. It might be expected that, when people are strangers to each other and must work together, fewer of their communications will be directed to specific individuals and more to their fellows in general. This will be true because communication to specific individuals presupposes some criterion for selecting them from among the others as the most suitable targets for manipulation. In a population of strangers, such a criterion is less likely to exist. This hypothesis is not stated explicitly in the literature on the acting crowd, but seemed to be a reasonable derivation from it.

The data on the Esterline-Angus tape included information about the targets of

<sup>13</sup> On this, and on all other observer codings of overt behavior, an inter-observer reliability coefficient of .75 or better was achieved for each coding category.

<sup>14</sup> This is an as yet unpublished technique developed by Keith Smith of the Department of Psychology of the University of Michigan.

<sup>15</sup> This machine was an Esterline-Angus Operation Recorder, Model A.W.

<sup>16</sup> Observers were taught the Bales' criteria for identifying targets of communication. See Robert F. Bales, *Interaction Process Analysis*, Cambridge: Addison-Wesley Press, Inc., 1950.



interaction. Each group was given a score consisting of the percentage of the total number of targets of its members' communication that were provided by the group as a whole instead of by particular individuals. The D groups exceed the A groups in percentage of communications directed toward the group as a whole at less than the .03 level of significance.

4. *Volume of Self-Oriented Needs Expressed.* Writers have often noted the submergence of the idiosyncratic needs of the individual member of the crowd. This seems to refer to a kind of one-for-all spirit prevailing in a time of emergency. In this circumstance, members of crowds do not make demands on their neighbors for assistance with specifically individual problems.

The volume of self-oriented needs expressed was recorded by using the observation form developed by Fouriez and others.<sup>17</sup> This form enables an observer to make reliable codings of the expression of needs classified as aggressive, dependent, and dominating, and of the general needs for release of non-specific tensions. This list is believed by its originators to provide a reasonably exhaustive code for self-oriented needs.

Since the volume of self-oriented needs expressed is, in part, a function of the volume of communication, group scores for volume of self-oriented needs were corrected for volume of communication. Based on these corrected self-oriented-needs scores, the chi-square test shows that D groups tend to express a smaller volume of self-oriented needs than do A groups at a level of confidence beyond .005.

5. *Fixity of Role Behaviors.* Once in motion, the behavior of people in acting crowds seems relatively fixed. People continue to play out the same role behavior with which they began as milling declined and a course of action was implemented. It is usually implied that this persistence of the structure of role behaviors comes from the urgency of carrying through with

some plan of action. Once committed to a plan, there is no time or opportunity for turning back and reconsidering.

Our data provide two kinds of observation that may be used to test this hypothesis. The first source of information lies in the Esterline-Angus records. From them, we may judge whether there is any appreciable shift over time in the rank order of the percentage of the total volume of communication supplied by each member of the group.<sup>18</sup> The A and D variations do not differ significantly by this test, both showing great variation in the rank orders over time.

The second source of information comes from another kind of observation of overt behavior—a method we have called "motor" observation. The observer of motor behavior kept a continuous record of all task-related acts involving a manipulation of the equipment. He recorded the number of times each group member shot at the golf balls, retrieved balls or pucks that went out of the playing area, and prevented balls or pucks from spinning out of reach. We may ask, then, how persistent were the rank orders of group members with respect to their specialization in each of these types of behavior. The analysis shows that Cell D groups exceed Cell A groups in persistence of a given role specialization at a point beyond the .05 level of confidence.

6. *Clarity of Differences in Role Behavior.* In the normal life of a society, a man often behaves like his neighbor, but he may also behave in quite different and specialized ways. In crowds, we are told, there is little role specialization. The members of crowds have relatively homogeneous roles. This picture usually refers to the period of milling. People are getting acquainted and getting organized. For a time, differences among them are vague, then some distinctions take shape.

No special predictions have been made about the extent of role differentiation once a course of action is being implemented. However, if common observation and current understanding of the nature of leadership

<sup>17</sup> Nicholas T. Fouriez, Max L. Hutt, and Harold Guetzkow, "Measurement of Self-Oriented Needs in Discussion Groups," *The Journal of Abnormal and Social Psychology*, 45 (October, 1950), pp. 682-690. This article should be consulted for more detailed definitions, conceptual and operational, of self-oriented needs.

<sup>18</sup> It is unfortunate that no funds were available for training observers in the Bales (see Bales, *op. cit.*) interaction coding system. Such a code would have reflected another dimension of specialization.

functions are correct, the reasonable prediction seems to be that members of crowds will be more specialized in behavior than will members of groups like those in Cell A. Degree of specialization, like emergence of leadership, is a function of the nature of the problems of the group. In the crowd, the urgency of the problem and the difficulty of its solution make more stringent the demands for coordination and for a maximum use of such skills as the members are demonstrated to have.

Some of our observations again come from the volume and motor observer records. The percentage contributed by each group member to the volume observations from his group was obtained. The sum of the inter-member differences in the size of these percentages is the measure of the size of the intra-group differences in behavior as reflected in this record. Similarly, the percentage contributed by each group member to the group total of behaviors on each category of the motor observer's record was calculated. Again, degree of specialization of behavior in the group is determined by the sum of the inter-member differences in the size of these percentages. No differences appear in a comparison of the A and D groups on the volume records, but the specialization of the D groups exceeds that of the A variation at a point beyond the .03 level of confidence on the motor observation data.

This last finding seems related to the greater rigidity of role structure in the D groups. Since there is less shift over time in what a man does, he has more opportunity to appear as more specialized in our measure of role differentiation. Such an interpretation is supported by the fact that this A-D difference in specialization on the motor records holds only for the total experimental period; not for either the first or second halves of that period taken separately.

The inter-member differences in specialization in the D groups are due almost entirely to the specialization of one person. The A and D variations do not differ in specialization if we use only the size of the difference between the two persons with the smallest percentage-specialization scores.

As before, data on this dimension were

expected from questionnaire responses. The second questionnaire contained this item:

Rate your two team-mates on the rating scales below. For each factor, there are two scales, one for each team-mate. Put a check mark to show how you feel about each team-mate on each rating scale.

Team-mate A is (circle): Red Yellow Blue  
Team-mate B is (circle): Red Yellow Blue

- a. *General Personality*: Consider his attractiveness as a person, congeniality, agreeableness, how much you would like him as a friend:

	Low							High
Team-mate A	—	—	—	—	—	—	—	—
Team-mate B	—	—	—	—	—	—	—	—
	0	1	2	3	4	5	6	7

- b. *Competence as a Leader*: Consider how well he planned, co-ordinated the team's work, etc.

	Low							High
Team-mate A	—	—	—	—	—	—	—	—
Team-mate B	—	—	—	—	—	—	—	—
	0	1	2	3	4	5	6	7

- c. *Co-operativeness*: Consider his willingness to do assigned tasks, take suggestions, co-operate, keep at his job, etc.

	Low							High
Team-mate A	—	—	—	—	—	—	—	—
Team-mate B	—	—	—	—	—	—	—	—
	0	1	2	3	4	5	6	7

- d. *Competence at Task*: Consider his skill in carrying out the task, ingenuity, manual dexterity, etc.

	Low							High
Team-mate A	—	—	—	—	—	—	—	—
Team-mate B	—	—	—	—	—	—	—	—
	0	1	2	3	4	5	6	7

No differences appear between the answers of A and D group members to these rating problems.

7. *"Goodness" of Task Performance*. No long discussion is necessary to defend the prediction that people who have less familiarity with a task will, other things being equal, do less well in executing it than will those with greater experience. The experimental situation provides one objective criterion of expertness—the number of successful shots actually made by group members and recorded by the motor observer. Members of Type A groups exceed the performance of Type D group members at a point beyond the .005 level of significance.

8. *Satisfaction with Other Group Mem-*

bers. The incipient hostility of crowd members toward each other as evidenced by their willingness to turn on each other in time of failure has often been observed. The usual explanation is that crowd members, faced with an urgent problem, are unusually anxious and tense about the performances of themselves and of others. Coupled with their rather poor performance in the present experimental situation, one might expect them to be less pleased with each other.<sup>19</sup> A questionnaire item taps this feeling at the end of the experiment. Subjects were asked:

Suppose you were to participate in another experiment in a few weeks that required three people to work simultaneously, just like this time. If we set up such groups, which would you prefer?

- Would like the same group as now.
- Would like to replace one of my two group members with some random person.
- Would prefer to be assigned to a different group.<sup>20</sup>

From these answers, members of D groups appear less satisfied with their colleagues than are members of A groups, at a point beyond the .05 level of confidence.

9. *Recall of Task-Related Events.* Blumer and others have tried to explain the reported difficulty of crowd members in recalling the things that happened while they were in the crowd. The common explanation seems to be that events in crowds are novel. As a

<sup>19</sup> The findings of John R. P. French (see his: "Organized and Unorganized Groups under Fear and Frustration," in Kurt Lewin and others, *Authority and Frustration*, Iowa City: University of Iowa Press, 1944, pp. 231-308) may seem to make for contrary expectations. It is possible that both predictions are correct, since the sources of irritation postulated are different. If frustration due to greater coordination is operative in our Type A groups, it seems to be less productive of lowered integration than the forces at work in our Type D groups.

<sup>20</sup> The reader's attention is called to the character of this questionnaire item. It is among the few such items that reflect a difference between the A and D variations. Unlike many of the other questionnaire items, it asks the subject to select from among clearly different behavioral choices. Perhaps the wider use of such a technique, instead of asking for judgments on rating scales in which the intermediate categories were not given specific labels, would have produced a larger number of significant results from the questionnaires.

result, such events are hard to classify under the usual categories of experience. This should mean that persons who are more familiar with the same task would also be more accurate in remembering their experiences with it.

As a very tentative approach to getting at such recall, subjects were asked to report the number of shots each had taken at the golf balls and to write down the scores obtained by his group in each of the five playing periods. (The experimenter permitted these scores to remain on the blackboard for only a few minutes after the close of the last playing period. The scores were erased long before subjects came to answer these questions at the end of an extensive questionnaire.) Cell A groups tend to exceed Cell D groups in accuracy of recall of number of shots taken at the golf balls at a point beyond the .15 level of confidence and to give more accurate reports of the scores received by the group at beyond the .10 level.<sup>21</sup> While these confidence levels do not meet the usual standards of significance, their proximity to significance and the consistency of their direction suggest that it would be worth while to pursue this methodology further.

10. *Recall of Non-Task Events.* The difficulty in getting the attention of crowd members so often reported by enforcement officials<sup>22</sup> has led to the hypothesis that the urgency and unfamiliarity of the crowd's problems makes for greater attention to those problems by its members. Thus less attention is available for events not related to the solution of the crowd's problems. The

<sup>21</sup> In making this comparison, the original scores on the number of times a subject shot at the golf balls were corrected for the fact that subjects in type A groups take more shots. This correction was made on the assumption that each additional shot adds one more unit of difficulty to the correct recall of the total number of shots made. The correction consists of adding or subtracting (as appropriate to close the gap between the number of shots observed and the number recalled by the subjects) the size of the average difference in number of shots between the A and D groups from the observed number of shots of the A groups. Sex was held constant in making this comparison.

<sup>22</sup> See, for example, Joseph D. Lohman, *The Police and Minority Groups*, Chicago: Chicago Park District, 1947.



attempt to test this hypothesis involved the tape recording in the adjoining room. Subjects were asked to fill out a multiple-choice test based on the content of that recording. The problem set by the test was one of correct recognition of the material reproduced from the tape. Analysis of the data gives no indication of a difference between A and D groups in the recall of this material. The within-group variation is great, as is that between groups, in each of the experimental variations.

#### GENERAL CONCLUSIONS

Ten predictions of differences between the A and D variations were made. Of these, 8 were given some support at or beyond the .05 level of confidence. By chance, we might have expected one of our predictions to be supported at or beyond that level. This strengthens the belief that, despite the small number of groups studied, the patterns revealed will continue to appear if the study is replicated. The findings of this study sug-

gest that the variables of degree of participants' acquaintance with the task and with each other as co-workers on the task are capable of explaining a large share of the phenomena observed in crowd behavior. Whether or not these variables are, in fact, at work in any given instance of the behavior of natural crowds is a separate problem.

The theoretical scheme used in this study is stated in concepts of high generality. This, in turn, frees our thinking to consider the possibility of finding these same phenomena in committees and families and legislatures and in any other social situation to the degree that the specified conditions are met. When we work along such lines, we stop thinking in terms of the phenotypic data from which our insights originally came and move more directly toward the construction of what Parsons<sup>23</sup> has called a general theory of social organization.

<sup>23</sup> Talcott Parsons, "The Prospects of Sociological Theory," *American Sociological Review*, 15 (February, 1950), pp. 3-16.

## SOCIAL CORRELATES OF RELIGIOUS INTEREST \*

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IN modern industrial societies it is clearly evident that individual interest in religion varies tremendously—from fanatical zeal at the one extreme to utter and complete indifference at the other. Yet despite the growing interest of sociologists and social psychologists in religious phenomena, the relationship of these variations to such

key sociological variables as sex, parenthood, occupation, wealth, income, education, vertical mobility, denominational affiliation, and so forth is still not clearly understood. While much has been written concerning these relationships, the empirical foundation on which these writings rest is extremely limited in most cases. The study described in this report was designed to provide further systematic data in this, as yet, relatively unexplored area.

#### SOURCE OF THE DATA

The data on which this report is based were gathered by the Committee on the Social and Psychological Factors Affecting Fertility, sponsored by the Milbank Memorial Fund. In 1941 this committee conducted a household survey of the white population of Indianapolis. Short schedules

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were completed for the white occupants of 102,499 dwelling units.

Shortly thereafter, an intensive follow-up study was made of a highly restricted sample of the population. In addition to the previous limitation on race, certain further restrictions were imposed on the sample. These were as follows:

1. Household must include a married couple.
2. Both husband and wife must be native born.
3. Both must have spent most of their married life in cities.
4. The couple must have been married in 1927, 1928, or 1929.
5. The wife must have been under thirty, and the husband under forty years of age, at the time of marriage.
6. Both husband and wife must be Protestant.
7. Both must have completed the eighth grade.
8. The marriage must be unbroken at the time of the interview, and neither spouse previously married.

On the basis of information obtained in the earlier survey, it was found that 2,589 families met all of these requirements. The data on which this present report is based were obtained in the intensive follow-up interviews with 860, or 33.2 per cent, of these families.<sup>1</sup>

The key question in the interviews, so far as the present study is concerned, was the query, "How much have you been interested in religion since marriage?" This question was asked in *separate* interviews of both husband and wife. All responses were coded in one of the following categories: 1) very much; 2) much; 3) some; 4) little; and 5) very little.<sup>2</sup>

Attitudinal questions of this sort frequently have been criticized by sociologists

<sup>1</sup>For details regarding the sampling procedure employed and the relationship of the sample to the universe, consult P. K. Whelpton and Clyde V. Kiser, "Social and Psychological Factors Affecting Fertility: The Sampling Plan, Selection, and the Representativeness of Couples in the Inflated Sample," *The Milbank Memorial Quarterly*, 24, pp. 49-93.

<sup>2</sup>In this report the distinctions between "very much" and "much" interest and "very little" and "little" interest have been omitted on the grounds that their inclusion adds nothing of importance to the analysis, and their omission simplifies the tabular presentation of the findings.

on several grounds. First, questions have been raised regarding the degree to which respondents employ the same frame of reference in interpreting the query itself.<sup>3</sup> Second, questions have been raised regarding the degree to which the gradations employed in coding responses (*e.g.*, "very much," "much," "some," *etc.*) have the same meaning for different respondents. Third, and most important of all, questions have been raised as to whether responses to such queries have any significant relationship to the non-verbal, non-interview situation behavior with which the investigator is primarily concerned.

Fortunately, data exist which make it possible to answer these objections with some degree of confidence. Of the 860 sample couples, 92 were childless. Both husbands and wives among these childless couples were asked not only how interested in religion they had been since marriage, but also the additional question, "How frequently have you attended church or Sunday School during your married life?" The relationship between the responses to this behavioral question and the responses to the attitudinal question on degree of interest in religion is presented in Table 1 below.

From an inspection of this table it may be seen that a very close relationship exists between the attitudinal and the behavioral variables. More than 70 per cent of those who rarely attended church or Sunday School reported "little" interest in religion. Similarly, more than 70 per cent of those who frequently attended church or Sunday School reported "much" interest in religion. A majority of those who reported irregular attendance responded that they had "some" interest in religion.

In view of this close relationship it would seem that the response to the query into degree of interest in religion since marriage may be regarded as a reasonably reliable measure of the individual's interest in the *organized* religious activities of the com-

<sup>3</sup>*E.g.*, with reference to the question regarding "interest in religion," it may properly be asked, "Do the respondents understand this question to be an inquiry into their interest in the institutional manifestations of religion, or merely an inquiry into interest in religion in the most vague and non-institutional sense of the term?"

TABLE 1. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY REPORTED FREQUENCY OF CHURCH OR SUNDAY SCHOOL ATTENDANCE SINCE MARRIAGE, IN PERCENTAGES

Reported Frequency of Attendance	N	Interest in Religion			
		Little	Some	Much	Total
Very seldom	46	71.7	28.3	0.0	100.0
Seldom or sometimes	102	31.4	54.9	13.7	100.0
Often or regularly	36	2.8	25.0	72.2	100.0

Chi-square=90.9; d.f.=4; P less than .001; C=.575.<sup>4</sup>

munity. More than that, it may be regarded as a reasonably reliable index of the individual's *participation* in such activities.

#### FINDINGS

*Sex.* Among the sociological variables related to religious interest and activity, none has been more widely recognized in American society than the sex variable. Novelists, playwrights, and preachers have all observed that women are generally more interested in the activities of religious groups and participate in them with greater frequency than men.<sup>5</sup> Systematic sociological research,<sup>6</sup> census data,<sup>7</sup> and denominational yearbooks<sup>8</sup> confirm these observations.

<sup>4</sup>Maximum possible C value for this table is .767.

<sup>5</sup>On occasion this observation has provided one of the major themes for a play or novel, as in the case of the best-seller and Broadway hit, *Life with Father*, by Clarence Day.

<sup>6</sup>*Cf.*, e.g., Louis Bultena, "Church Membership and Church Attendance in Madison, Wisconsin," *American Sociological Review*, 14, pp. 384-389; Joseph Fichter, "The Profile of Catholic Religious Life," *American Journal of Sociology*, 58, pp. 145-149; Anonymous, "How Important Religion Is to Americans," *Catholic Digest*, 17 (February, 1953), pp. 6-12; or A. B. Hollingshead, *Elmtown's Youth*, New York: John Wiley & Sons, Inc., 1949, p. 460.

<sup>7</sup>*Cf.* United States Bureau of the Census, *Religious Bodies: 1936*, vol. I, p. 23.

<sup>8</sup>*Cf.* e.g., the denominational yearbooks of groups such as the Congregational-Christian Church and the Augustana Lutheran Church which give

As may be seen in Table 2 below, the data gathered in Indianapolis provide further evidence in support of the hypothesis of greater interest among women. Sixty per cent more women than men expressed "much" interest in religion since marriage.

This variation in religious interest along sex lines has been more often observed than analyzed. The traditional explanation that "women are by nature more religious than men" is hardly satisfactory from the sociological point of view. Yet sociologists have failed to provide a more satisfactory explanation.

From the sociological point of view, this difference in religious interest would seem to be directly related to the very different social environments in which the average husband and the average wife in American society live, and the variations in the demands made on the individual in those differing environments. The average husband spends the major part of his waking hours in the highly competitive job world. The average wife, by contrast, spends the major part of her waking hours in the relatively protected, non-competitive world of family activities.

These very different environments tend

statistical breakdowns of denominational membership by sex.

<sup>9</sup>The summary score, which is presented for the convenience of the reader, was calculated according to the following formula:  $100 + M - L$ . A score of 200.0 represents maximum interest, while a score of 0.0 represents minimum interest.

TABLE 2. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY SEX, IN PERCENTAGES

Sex	N	Interest in Religion				Summary Score <sup>9</sup>
		Little	Some	Much	Total	
Male	860	33.5	42.6	24.0	100.1	90.5
Female	860	19.8	41.8	38.4	100.0	118.6

Chi-square=60.2; d.f.=2; P less than .001.



to produce quite different, though not incompatible, personality types. Successful adjustment in the job world not uncommonly requires the development of personality traits and behavior patterns which conflict with basic Christian ethics. Successful adjustment in the family relationship, which is of relatively greater importance for the wife, places a premium on the development by the wife of just those personality traits and behavior patterns stressed by the churches (*i.e.*, altruistic attitudes and actions).<sup>10</sup>

*Parenthood.* A second variable investigated was that of parenthood. On the basis of previous research, there seemed little indication that a significant difference in religious interest among Protestants might be found when childless couples were compared

In attempting to account for this relationship, the problem of causality must be faced. To the present writer it seems not unreasonable to regard both factors (parenthood and religious interest) as causal under certain conditions.

Since the Protestant churches generally share with the Catholic church the view that parenthood should be the natural consequence of marriage (though Protestants usually reject the Catholic view that there must be no "artificially" established limit to family size), devout Protestants would probably have greater difficulty in rationalizing a childless marriage than would those whose ties with the church were more tenuous. In this sense, religious interest may function as the independent variable, and parenthood as the dependent variable.<sup>11</sup>

TABLE 3. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY COMPOSITION OF FAMILY UNIT, IN PERCENTAGES

Composition of Family Unit	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
No living children	184	35.9	42.4	21.7	100.0	85.8
One or more children	1,536	25.5	42.1	32.3	99.9	106.8

Chi-square=12.0; d.f.=2; P less than .003.

with those with children. Since, however, the pattern of married life is so profoundly altered in many ways by the arrival of children, it seemed that the relationship of this factor to degree of religious interest should be examined.

As an inspection of Table 3 indicates, a highly significant difference was found between the degree of religious interest expressed by couples with children and those without. Half again as many of the couples with children reported "much" interest in religion as compared with the childless couples.

<sup>10</sup> Additional data gathered in this study, which cannot be presented here due to limitations of space, indicated that sex differentiation with respect to interest in religion begins during childhood when the individual is preparing for his adult roles in society. Apparently the tendency for conflicts to develop between the requirements of the male role and the role of church member is more or less consciously recognized by parents and results in greater laxity in the religious training of boys than is characteristic of that of girls.

Casual observation has suggested to the writer, however, that once children arrive there is often a quickening of religious interest on the part of the new parents. This frequently develops when the problem of transmitting the cultural heritage to the children is faced. Under such conditions religious interest may function as the dependent variable and parenthood as the independent variable.

*Occupation.* A third major variable investigated was that of the occupational role of the family head. Despite the recognized importance of this variable as an influencing factor in the behavior of family members generally, only a beginning has been made in the task of determining how variations in

<sup>11</sup> All of the couples in the sample employed in this research who were childless were childless by choice, not by necessity, so far as available evidence would indicate. This additional limitation on the sample was imposed for reasons relevant to the research conducted by The Committee on the Social and Psychological Factors Affecting Fertility.

the occupational role of the family head are related to variations in the religious behavior and attitudes of family members.

Stratification-oriented studies of American communities have indicated that individual Protestant congregations tend to be class-typed, and that the occupational role of the family head plays an important part in determining the particular congregation with which a family unites.<sup>12</sup> However, with respect to the present problem of whether interest in religion in general (as opposed to interest in a specific congregation or denomination) varies with differences in the occupational role of the family head, the evidence is unclear.<sup>13</sup>

tions were so slight that they could not safely be assumed to be the result of factors other than chance.

While an analysis employing the gross categories of the Edwards classificatory scheme suggests that variations in religious interest are unrelated to the occupational structure of American society, a more detailed analysis employing more homogeneous categories pointed to the opposite conclusion. For example, when unskilled workers were divided into service workers and laborers, a highly significant difference ( $P < .01$ ) in degree of religious interest was found. Half of the service workers and their wives ( $N = 48$ ) expressed "much" interest in re-

TABLE 4. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY LONGEST OCCUPATION OF HUSBAND, IN PERCENTAGES

Longest Occupation	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Professional	168	29.3	32.9	37.8	100.0	108.5
Managerial *	222	33.3	33.3	33.3	99.9	100.0
Clerical-sales	406	26.4	45.1	28.6	100.1	102.2
Skilled	358	22.1	44.4	33.5	100.0	111.4
Semi-skilled	466	25.3	46.6	28.1	100.0	102.8
Unskilled **	88	30.7	35.2	34.1	100.0	103.4
Others and N.A.	12	33.3	50.0	16.7	100.0	83.3

\* Managers, proprietors, and officials.

\*\* Laborers and service workers.

Chi-square=10.2; d.f.=10; P more than .4.

When the Indianapolis couples were classified according to Edwards' scheme, the striking feature was the absence of significant variations in religious interest among the several categories. In fact, the varia-

tion, while only 15 per cent of the laborers and their wives ( $N = 40$ ) expressed a similar degree of interest.

Marked variations were found within the other major occupational categories. In those professional families in which the husband was employed as an engineer, chemist or other "technical" professional worker, 43 per cent of the respondents ( $N = 44$ ) expressed "much" interest in religion. By contrast, in those professional families in which the husband was a doctor, dentist, lawyer, or college professor, only 27 per cent of the respondents ( $N = 36$ ) expressed such interest.

Among clerical and sales workers it was found that 36 per cent of the store clerks and their wives ( $N = 106$ ) expressed "much" interest in religion, while only 25 per cent of the travelling salesmen and their wives ( $N = 44$ ) expressed a similar degree of interest. Among the skilled workers it

<sup>12</sup> Cf., e.g., Walter Goldschmidt, "Class Denominationalism in Rural California Churches," *American Journal of Sociology*, 49, pp. 348-355; Liston Pope, *Millhands and Preachers*, New Haven: Yale University Press, 1942; Hollingshead, *op. cit.*, pp. 248-250; or Bultena, *op. cit.*

<sup>13</sup> The recently published report of the *Catholic Digest* survey, *op. cit.*, suggests that there is almost no difference in the degree of religious interest among the different categories of urban workers. Bultena's study of Madison, Wisconsin, *op. cit.*, points to a similar conclusion. On the other hand, Hollingshead's study of Elmtown, *op. cit.*, revealed a marked relationship between religious interest and prestige status (which was shown to be closely related to occupational rank). Frequency of church membership varied directly with status level (p. 459), while frequency of church attendance was found to be greatest in the middle classes (p. 98).

was found that 50 per cent of the respondents in printers' families ( $N = 32$ ) were "much" interested in religion, while only 29 per cent of the machinists and their wives ( $N = 42$ ) indicated as much interest.

While none of these differences are statistically significant, except that between service workers and laborers, this writer is inclined to believe that it would be unwise to regard all such differences as simply the product of chance variation until further evidence is gathered. It seems not unreasonable to believe that the peculiar characteristics of certain occupational roles in American society are more consistent with or more conducive to religious interest than others.

*Financial status.* If variations in religious interest are related to differentiation in the secular structure of society, a relationship between the financial status of families and their interest in religion might be expected in a society such as our own in which economic considerations are so important. Most previous research in the area suggests that this is the case, although the nature of the relationship has not been made clear.<sup>14</sup>

<sup>14</sup> Hadley Cantril, "Educational and Economic Composition of Religious Groups: An Analysis of Poll Data," *American Journal of Sociology*, 48, pp. 574-579, found in his analysis of a national sample that there was a direct relationship between economic status and frequency of church membership. Bultena, *op. cit.*, found a direct relationship between house value and frequency of church membership in Madison, Wisconsin. Both of these studies indicated, however, that the frequency of church membership did not vary greatly between the several economic levels.

Hollingshead, in his study of Elmtown, *op. cit.*, found, as did Cantril and Bultena, a direct relationship between frequency of church membership and prestige status (which was closely related to financial status). He also found, however, that the incidence of church attendance (which would seem to be a better measure of religious interest) was greatest in the middle classes. This would seem to suggest that church membership in the upper classes is less frequently the consequence of interest in religion *per se*, and more often the consequence of other interests (e.g., a community control interest), than is the case in the middle classes.

By contrast with the studies mentioned above, the recent survey conducted by the *Catholic Digest*, *op. cit.*, revealed almost no difference in the degree of importance attached to religion on the several income levels. This writer is inclined to question the significance of this finding due to the failure to employ regional and denominational (*i.e.*, Protestant, Catholic, and Jewish) controls in analyzing

In the present study, data were obtained both on the net worth<sup>15</sup> of the sample families and on their income. Analysis of the relationship of each of these variables to religious interest indicated that the greatest interest was to be found among the families in the middle economic range. The relationship between net worth and religious interest was so ill-defined, however, that the chi-square test indicated that the null hypothesis could not safely be eliminated ( $P > .08$ ).

The relationship between average annual income and religious interest was somewhat more pronounced. From an inspection of Table 5 it may be seen that those families which had enjoyed an average annual income of between \$1,200 and \$2,999 expressed "much" interest in religion more frequently than did those with either larger or smaller incomes. The chi-square test indicated that the difference in religious interest between the \$1,200-2,999 category and the \$3,000 and more category was significant at the one per cent level, while the difference between the \$1,200-2,999 category and the under \$1,200 category was significant at the two per cent level.

The tentative conclusion to which the present study points is that the middle class (when defined in purely financial terms) exhibits greater interest in religion than either the upper or lower classes. If, however, this generalization is sound, two important qualifications must be added. First, the middle class when defined in these terms is a most inclusive category. More than 70 per cent of the sample families were included in the group which had an average annual income of between \$1,200 and \$2,999. Second, the difference in the degree of religious interest between the middle category and the upper and lower categories, while statistically significant, is not great. For example, in the middle income category only 33.3 per cent of the respondents

income data. Their findings suggest the need for such controls.

<sup>15</sup> Net worth was measured in terms of the difference between assets and liabilities. More specifically, it was the sum of cash savings, market values of equities in real property, investments in business enterprises, and insurance policies, minus outstanding debts. Net worth was not determined on the basis of a response to a single question, but rather on the basis of responses to a series of questions pertaining to each of the component elements.



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TABLE 5. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY AVERAGE ANNUAL INCOME, IN PERCENTAGES

Average Annual Income	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
\$3,000 or more	136	38.2	35.3	26.5	100.0	89.3
\$2,400-\$2,999	128	22.8	39.4	37.8	100.0	115.0
\$2,000-\$2,399	194	27.3	38.6	34.0	99.9	106.7
\$1,800-\$1,999	154	26.0	42.2	31.8	100.0	105.8
\$1,600-\$1,799	166	19.3	48.2	32.5	100.0	113.2
\$1,400-\$1,599	284	27.1	40.1	32.7	99.9	105.6
\$1,200-\$1,399	290	22.1	45.2	32.8	100.1	110.7
\$1,000-\$1,199	210	30.5	44.8	24.8	100.1	94.3
Less than \$1,000	156	29.5	43.0	27.6	100.1	98.1
Not ascertained	2	0.0	100.0	0.0	100.0	100.0

Chi-square=28.1; d.f.=16; P less than .05.

ents expressed "much" interest in religion, while in the higher and lower income categories 26.5 per cent and 26.0 per cent, respectively, indicated just as great an interest.

**Education.** Another major variable associated with many behavioral and attitudinal differences in American society is the degree of formal education obtained by the adult members of the family unit. With respect to the relationship between extent of formal education and degree of religious interest, three plausible hypotheses suggest themselves. All have some support in previous empirical research.

First, as a derivative of the "middle class conformity hypothesis" it might be predicted that the greatest degree of religious interest would be found in the middle educational groups. Hollingshead found evidence to support this hypothesis in Elmtown, as did West in Plainville.<sup>16</sup>

A second hypothesis which suggests itself is that the degree of interest in religion varies *inversely* with the degree of formal

education. This hypothesis would be consistent with the doctrine of logical positivism and the viewpoint of the evolutionists that religious beliefs are a survival from man's more primitive past, and thus destined to disappear with increasing knowledge of the nature of the world in which man lives. This hypothesis would seem to be supported by the recent survey conducted by the *Catholic Digest*.<sup>17</sup>

A third hypothesis which must be considered is the hypothesis that interest in religion varies *directly* with the degree of formal education. This hypothesis may be derived from the more general finding of recent research which indicates that participation in formal voluntary associations tends to vary directly with status. Participation is presumably not unrelated to interest. This hypothesis has been supported in studies of church membership by Cantril and Bultena.<sup>18</sup>

<sup>17</sup> *Op. cit.*; this finding is probably due in part to the failure to control for denomination when analyzing the data on the relationship of educational attainment to degree of importance attached to religion.

<sup>18</sup> Cantril, *op. cit.*; Bultena, *op. cit.*

<sup>16</sup> Hollingshead, *op. cit.*, Chap. 5; James West, *Plainville, U. S. A.*, New York: Columbia University Press, 1945, p. 130.

TABLE 6. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY EDUCATIONAL LEVEL ATTAINED, IN PERCENTAGES

Highest Educational Level Attained	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
College	320	28.8	40.3	30.9	100.0	102.1
High school	1,035	25.5	44.3	30.1	99.9	104.6
Grammar school	364	28.0	37.6	34.4	100.0	106.4

Chi-square=6.9; d.f.=4; P more than .1.

TABLE 7. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY SUCCESS IN COMPLETING HIGH SCHOOL OR COLLEGE, IN PERCENTAGES

Educational Success	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Graduates	614	24.9	39.9	35.2	100.0	110.3
Non-graduates	733	28.0	45.4	26.6	100.0	98.6

Chi-square=16.2; d.f.=2; P less than .001.

Data gathered in Indianapolis fail to provide support for either hypotheses one or three. They tend to support hypothesis two, but as may be seen from an inspection of Table 6, the differences in religious interest by educational level are so slight that the null hypothesis cannot be rejected.

While no significant relationship was found to exist between *general* educational level attained and degree of religious interest, a *year-by-year* breakdown of the educational data revealed a significant relationship between religious interest and success or failure in completing a given unit of education. College graduates expressed greater interest in religion than did those who had attended college but failed to graduate. The same was true of high school graduates and non-graduates. Those who had completed grammar school expressed greater interest than did those who had gone on, but had failed to graduate from high school.

Although this relationship is statistically significant, it is not easily explained. One possible hypothesis which occurs to the writer is that successful completion of a given unit of education is related to the degree of an individual's integration into the life of the community. Those who are more fully integrated would be expected to par-

ticipate more fully in voluntary corporate activities, both religious and secular, and through participation develop greater interest. Such persons would also be expected to achieve the educational goals appropriate to their status level with greater frequency than those less thoroughly integrated. The latter might be expected to fail more often in their efforts to achieve this goal, since lack of integration would contribute to a lack of awareness of the norm or of the importance attached to it, and also to a relative imperviousness to informal community sanctions supporting the norm. This interpretation, however, is highly speculative.

*Vertical mobility.* While the relationship between the static aspects of the status system (as indicated by the data on occupational status, net worth, income, and education) and religious interest are not pronounced, the Indianapolis data pointed to a marked relationship between intra-generational income mobility and religious interest.

As may be seen from an inspection of Table 8, the degree of religious interest tended to vary *inversely* with degree of upward-mobility. Those who had enjoyed the greatest income gain since marriage expressed the least interest in religion, while those who had suffered an income loss during

TABLE 8. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY INTRA-GENERATIONAL INCOME MOBILITY SINCE MARRIAGE, IN PERCENTAGES

Mobility Status	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Maximum income gain	172	38.6	36.8	24.6	100.0	86.0
Medium income gain	436	30.0	41.3	28.7	100.0	98.7
Small income gain	774	24.4	44.3	31.3	100.0	106.9
Stable income	192	22.9	41.1	35.9	99.9	113.0
Small income loss	92	18.5	38.0	43.5	100.0	125.0
Maximum income loss	46	19.4	45.7	34.8	99.9	115.4

Chi-square=29.1; d.f.=10; P less than .002.

this period indicated the greatest degree of interest.<sup>19</sup>

Here again, it is less difficult to demonstrate the existence of a relationship than to account for it. Marx's thesis that religion is an opiate might seem relevant here, since those who have been the "failures" with reference to income express a greater interest in religion than do the "successes." Such a view, however, presents some difficulties when an attempt is made to reconcile it with the previous finding of greater interest among those who successfully completed a given unit of education than among those who failed to achieve this important life goal.

Possibly a more plausible explanation might be achieved through an analysis of the requirements which are generally necessary for economic advancement in contemporary American society, and the relationship of those requirements to the requirements of active church membership. Economic advancement generally demands long hours devoted to the job, which would result in a conflict with the role of active church member. Also, if the work of Sutherland and others on white collar crime and its frequency is at all valid, it would seem that success in the job world not uncommonly may depend upon the individual's willingness to violate legal codes and Christian ethics. Thus, as a result of such role-conflicts, the would-be successful individual is often forced to break his ties with one or the other of the conflicting groups. Whether the break is with the business group or with the church, the result is the same—a significant inverse relationship between economic advancement and religious interest. A "role-conflict" hypothesis of this sort

would be consistent not only with the vertical mobility data, but also with the evidence indicating greater interest in religion among women than among men.

*Rural vs. urban origins.* Past research has revealed an inverse relationship between community size and religious interest and activity.<sup>20</sup> It has not been made clear, however, whether these differences have a lasting effect on those raised in different environments, or whether migrants to the cities from rural areas adopt the dominant urban cultural patterns after migration.<sup>21</sup>

As may be seen from an inspection of Table 9, the data gathered in the present study fail to provide a clear answer to this question. Those who were raised on farms expressed somewhat greater interest in religion than did those not raised on farms. The difference, however, is not statistically significant. The findings suggest that influences of the urban environment on rural migrants may be stronger than influences of the farm background. In view of the absence of adequate controls, however, this conclusion is most tentative.

*Denominational preference.* The final major variable to be considered is that of denominational preference. Variations in the degree of religious interest among the adherents of the three major faiths is widely recognized. Catholics are generally credited with being the most zealous in their religious interest, while Jews are thought to exhibit the greatest degree of indifference. Limited research in this area has suggested that some important differences may also exist *within* the Protestant group.<sup>22</sup>

<sup>19</sup> The relationship of *inter-generational occupational mobility* to religious interest was also examined, though no significant difference in interest was discovered. The summary scores for the three mobility categories were: up-mobiles, 100.9; non-mobiles, 105.3; down-mobiles, 101.2. The null hypothesis could not be rejected since the probability of such a relationship occurring by chance was greater than .4.

The apparent inconsistency between this finding and the finding presented in Table 7 and the text above suggests the need for intensive research into the problem of the interrelationship of the several commonly employed techniques for operationalizing the concept "vertical mobility."

<sup>20</sup> Cf., e.g., William F. Ogburn with Clark Tibbitts, "The Family and Its Functions," in *Recent Social Trends* prepared by the President's Research Committee on Social Trends, New York: McGraw-Hill Book Co., 1938, p. 661 ff.; Frederick A. Bushee, "The Church in a Small City," *American Journal of Sociology*, 49, pp. 223-232; H. Paul Douglass and Edmund deS. Brunner, *The Protestant Church as a Social Institution*, New York: Harper and Brothers, 1935, pp. 39-41; or John L. Thomas, "Religious Training in the Roman Catholic Family," *American Journal of Sociology*, 57, pp. 181-182.

<sup>21</sup> Or perhaps it may be the rural migrants share the urban view as the result of selective migration of those least interested in religion among the rural population.

<sup>22</sup> Cf. Bultena, *op. cit.* or the *Catholic Digest* survey, *op. cit.*



TABLE 9. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY FARM VS. NON-FARM BACKGROUND, IN PERCENTAGES

Background	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Farm	358	23.8	40.8	35.5	100.1	111.7
Non-farm	1,362	27.4	42.5	30.1	100.0	102.7

Chi-square=3.4; d.f.=2; P more than .1.

This was the case in Indianapolis, as the data in Table 10 indicate. Those who recorded a preference for the Pentecostal bodies ranked highest in interest, followed by the Lutherans, the Evangelical and Reformed and the Christian Scientists. At the other extreme, the Episcopalians, Quakers, and United Brethren expressed the least interest in religion.

On first inspection, the status variable seems to afford a satisfactory explanation for the variation in religious interest by denomination. Obviously the Pentecostal groups recruit the vast majority of their members from the lower strata of society, while the Episcopal church has been noted for its singular attraction for the socially elite. This suggests that denominational variations in religious interest are simply a function of the social status of the members of the group.

Unfortunately, however, when the analy-

sis is carried beyond the extreme cases, this relationship is no longer apparent. For example, the Lutherans and the Christian Scientists, who ranked well ahead of the Methodists and United Brethren in religious interest, also enjoy a higher social status in Indianapolis, if that may be judged by such status-related variables as income, education, net worth, occupation of husband, and rental value of home.

Another indication of the limitations of the status variable as an explanation of the variations in religious interest by denomination can be seen in the difference in interest expressed by members of the Methodist group on the one hand, and the Disciples of Christ on the other. With respect to the above-mentioned criteria of status, the two denominations are almost identical. Yet the difference in religious interest expressed by the members is quite marked. The chi-square test indicates that it is so great that the

TABLE 10. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY DENOMINATIONAL PREFERENCE, IN PERCENTAGES

Denominational Preference	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Pentecostal	26	7.7	7.7	84.6	100.0	176.9
Lutheran	85	20.0	25.9	54.1	100.0	134.1
Evangelical and Reformed	55	16.4	38.2	45.4	100.0	129.0
Christian Scientist	53	18.9	35.8	45.3	100.0	126.4
Nazarene	25	20.0	36.0	44.0	100.0	124.0
Baptist	188	19.1	50.5	30.3	99.9	111.2
Disciples of Christ	296	20.6	48.0	31.4	100.0	110.8
Presbyterian	149	28.2	40.9	30.9	100.0	102.7
Unitarian, etc.*	17	17.6	64.7	17.6	99.9	100.0
Methodist	451	27.1	46.6	26.4	100.1	99.3
United Brethren	36	30.6	41.7	27.8	100.1	97.2
Quaker	17	35.3	41.2	23.5	100.0	88.2
Episcopalian	21	47.6	33.3	19.0	99.9	71.4
Miscellaneous	66	19.7	25.8	54.5	100.0	134.8
No preference	115	53.0	37.4	9.6	100.0	56.6
Not ascertained	119	42.0	37.0	21.0	100.0	79.0

\* Includes Congregationalists and Universalists.  
Chi-square=146.3; d.f.=26; P less than .001.

null hypothesis can safely be ignored ( $P < .001$ ). Thus, while denominational status differences may be one factor involved, they clearly do not provide a full or complete basis for explaining these denominational variations in interest.

A second factor which may account for a part of this variation is the stress, or lack of it, placed on theological considerations in the several denominations. All of the five leading denominations are ones in which theological considerations are strongly stressed (though in varying fashion).

A third factor which may contribute to systematic variations in religious interest along denominational lines is the varying importance attached to the religious education of the young. This factor, while undoubtedly related to the previously men-

tended to be selective on the basis of either ethnic loyalty or religious loyalty or both. Those immigrants or their children who lacked either or both of these loyalties tended to drift out of these denominations and very often into other, "more American," denominations. Thus, the element which remained in these groups tended to include an unusually high proportion of intensely interested individuals. This selective factor may very well have produced consequences which will endure for an extended period of time.

*Mixed marriages.* The great importance of denominational ties was further indicated by an analysis of the degree of religious interest expressed by those who were married to spouses with similar denominational ties at the time of the interview, as con-

TABLE 11. DEGREE OF INTEREST IN RELIGION SINCE MARRIAGE, BY DEGREE OF RELIGIOUS HOMOGENEITY IN FAMILY UNIT, IN PERCENTAGES

Type of Marriage	N	Interest in Religion				Summary Score
		Little	Some	Much	Total	
Mixed marriage	302	33.1	41.7	25.2	100.0	92.1
Unmixed marriage	1,146	20.8	43.9	35.2	99.9	114.4

Chi-square=23.2; d.f.=2; P less than .001.

tioned factor of theological emphasis, deserves separate mention since it is probably to some degree an independent variable. Certain denominations, particularly those of continental European background, have laid much greater stress on the religious training of the young than is characteristic of denominations of British or American origin. For example, in some Lutheran parishes parochial schools are maintained, and in virtually all parishes there is some provision for special weekday training of children for one, two, or more years preceding confirmation. Such systematic training undoubtedly has an effect.

Finally, the peculiar ethnic character of certain denominations, particularly those of continental European background, has probably been a factor also. Many persons with Lutheran, Evangelical, or Reformed membership in Europe came to this country, but did not affiliate with these groups after arrival. Membership in these denominations

trasted with those then married to spouses with differing ties. As may be seen in Table 11, the proportion of persons expressing "much" interest in religion was significantly greater among those whose spouses shared their denominational preference, than among those married to partners with differing ties.

These findings are also of interest since they emphasize the importance of the family unit as a factor influencing religious interest. The reinforcing tendencies which result from religious unity within the family seem to be of great importance. Conversely, the absence of such mutual reinforcement frequently seems to be associated with a relative lack of interest in religion. The data on mixed marriages together with those on childlessness suggest the great importance of the family unit as a factor influencing religious attitudes and behavior, and the need for further research in the area.

## CONCLUSIONS

The specific conclusions relating to particular variables and their relationship to religious interest have been stated in the body of this report. Thus, only general conclusions remain.

To begin with, it should be noted that at the present time there are but few generalizations about the social correlates of religious interest which sociologists can state with any real assurance. There are even fewer which can be made about the causative factors which lead to intense interest on the part of some and gross indifference

on the part of others. The foundation for such generalizations in terms of systematic empirical evidence is, at present, far too limited. With few exceptions, the generalizations which sociologists make in this area at the present time must be recognized as little more than speculations—at best they may be called scientific hypotheses with a low degree of probable validity. This situation can be remedied only through continued research involving both careful rechecking of prior findings and the systematic follow-up of leads suggested by previous research or existing theory.

## ETHICAL LIMITATIONS ON SOCIOLOGICAL REPORTING

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**I**N his primary task as the discoverer of new knowledge, the modern scientist is governed by the obligations to search for truth, to be objective, to discern the relevant, to check meticulously his data, and, in some circles, to accept responsibility for the use to which his knowledge is put. This ethical code, however, fails to cover the problems arising from the relations between the scientist and the objects of his observation and experimentation. This may be due in part to the very conceptualization of phenomena as "objects." Only "subjects" have rights which must be respected.

There is evidence, of course, that social scientists are vaguely aware that they incur responsibilities which extend beyond the procedural ethics of science itself: that men are subjects as well as objects, and that even when studied as objects they retain certain of their rights to privacy and respect. Thus the experiments on living human bodies of prisoners, made by Nazi doctors, gained them infamy rather than fame. The theoretical literature of American psychiatry hides the identity of most of its patients. And sociologists and anthropologists frequently attempt to disguise the communities they study.

The lack of consensus in this area of re-

sponsibility attests to the fact that the norms underlying such efforts to respect people who are studied have never been systematically formulated as part of the procedure of scientific research and reporting. Indeed individuals and groups receive the greatest protection when scientific research is linked with the doctor-patient relation as in the case of psychiatry. In other areas protection seems to depend upon a diffuse and uncertain feeling of respect for the human "object." This protection is adequate, however, only where it does not interfere seriously with the gathering and reporting of data or where its violation would take such extreme form as to severely shock both the scientist and his society.

Under present conditions, the possibility of disturbance and shock seems greatest where research and reporting directly involve identifiable small groups and individuals. Research workers also seem to be effectively barred from experimentation which threatens the physical wholeness of the individual. Beyond these areas of investigation every research worker seems to be largely on his own in determining what research shall be conducted and what report shall be made so far as the impact of the research and the report on the objects of the study are concerned.



In this state of normlessness even the individual and the small group can be threatened if the possibility of identification is only indirect or if the violation of rights is not obvious and flagrant. Thus men may not be plunged into freezing water involuntarily, but children have been placed in authoritarian situations to discover the effects on their attitudes and behavior. Sexual relations between husband and wife cannot be observed by the family sociologist, but other forms of private behavior have been observed and reported. The psychiatrist will guard the identity of his patient, but the student of a community may report behavior on the part of an individual who can be indirectly identified by other members of the community or by other people in the larger society.

Although the psychologist and the social psychologist face ethical problems in experimenting with human beings, the sociologist seems most vulnerable in his studies of small groups and communities. His problem, since he does not often experiment, seems to be the question of whether there are ethical limitations on the "complete" objectivity of a research report, concerning such groups and communities, for it is in this area of research that there are signs that the ethical sense of the sociologist is either dormant or only intermittently and uncertainly active. An explicit code of ethics which will govern the social scientist in reporting such data seems urgently needed.

In attempting to develop a system of relational ethics the sociologist must remember that while the people he studies have rights, these rights cannot be secured by an unqualified assertion of the "subject" status of his objects of investigation. It is an obvious absurdity to assert that these "subjects" are entitled to absolute anonymity, privacy, and protection, for in various circumstances the sociologist may be obligated to describe in full detail the actions of identifiable groups and individuals. Moreover the development of a code of ethics will not relieve the sociologist of moral choice, but can serve only as a guide for the making of decisions for which he must accept responsibility. Having said all this, however, it remains true that sociologists need to formulate a system of ethical norms to protect the objects of sociological reporting. It is as a tentative state-

ment of the conditions relevant to such norms and of a few of the most important norms themselves that the following discussion is offered.

Before presenting our conception of some of the important normative variables in the formulation of such a system of ethics, it is necessary to consider first the matrix of conditions into which the system must be placed. Two aspects of this matrix seem particularly important. The first of these has to do with the various groups of people to whom the reporting sociologist has obligations; the objects of the study are only one such group. The second aspect concerns the fact that even in community and small group studies certain *kinds* of data and certain *modes of data presentation* pose the ethical problem in its most intense form, while other data and modes of presentation offer only minor problems. It is necessary to distinguish these factors, since, as scientists, maximum freedom is desirable and hence no needless restrictions are in order.

In preparing a research report on a small community or group the sociologist has a moral duty toward several different groups. Because his obligations to each of these differ in kind and degree while at the same time they condition and limit one another, it seems necessary to set forth briefly the categories into which they fall.

1. For practical, as well as moral reasons, the sociologist must consider the wishes and needs of those persons who have allowed, invited, sponsored, or cooperated with the study. Management of a factory group, officers of a labor union, ministers, and city officials, are all examples of people who may have some concern for the results of sponsored research. The sociologist's obligations to such persons are truthfulness, the honoring of confidences, scientific objectivity, and honest reporting.

2. The sociologist has obligations to the source from which research funds were obtained. Like anyone who enters a contractual agreement, he has the ordinary obligations to employ these funds honestly and usefully, and to abide by the terms of the agreement concerning publication and ownership of data, and by any other explicit provisions which might have been incorporated in the contract.

3. The publisher of the research report

has a call upon the moral consideration of the sociologist. Again the obligations are derived from the ordinary desirability of honesty and thoroughness, or from legal rights relating to libel suits and other embarrassments in which the report may involve the publisher.

4. Social scientists in general may be said to have a claim on the findings of the social researcher. The scientist's colleagues have a moral expectation that the findings will be made available to them in a serious, honest, and competent report. In addition to these expectations which do not differ much from expectations of professionals in other areas of work, there are the specifically scientific demands for a free exchange of data and knowledge unhindered by secrecy and suppression.

5. Another kind of group has a similar claim, perhaps not on the individual scientist but certainly on the discipline, to receive the findings of social research. In the long run this group is the society itself, for it is particularly important that social science knowledge ultimately become the possession of all the people. If there are reasons for the holding back of research findings from the general public for a short time, this group will still contain, at a minimum estimate, the key persons in a community or group who are in a position to utilize the research findings in programs of social improvement. The sociologist himself must bear the responsibility for determining who these persons are, unless they are defined by legal norms of the community of which the social scientist himself is a member.

6. Against the claims of all these groups on the findings of the sociologist, there exist the rights of the community studied, its subgroups, and its individual members. Their rights to secrecy, privacy, reputation, and respect, will vary according to circumstances and to the demands of the other groups, but they are intrinsically present in a society like our own which in its central tradition accords dignity and worth to the individual. The sociologist has not discharged his duties when he has met his obligations to sponsors, fund sources, publisher, social scientists, and the general public; nor has he completely discharged them when he makes a perfunctory effort at disguise, ambiguity, or anonymity. He is always faced with the

moral problem of how much to tell about the lives and habits of the members of the community or small group.

The problem varies in its intensity, however, with the kind of data and with the mode of presentation. It seems obvious that historical material allows more latitude for reporting than contemporary material. Every study of a small group or community seems to require a brief sketch of historical background, and through this research the scientist may discover certain skeletons in the closet. Their revelation may be pertinent to the understanding of the group and will probably not intrude too greatly upon the community's or its individuals' reputations.

Within the area of contemporary material a distinction can be drawn between studies of primitive societies and civilized communities. It is to be supposed that the details of social life among the Samoans were not reported to these people, and if any reputations suffered from such study it was only among non-Samoans. There have been instances, however, of anthropologists' reports getting back to American Indian tribes, causing some dissension and suspicion among the members of the tribe. In either case the sociologist must consider these people as the subjects of human rights, even though the prospect of moral damage may not be great.

In studying contemporary communities the problem of reporting varies according to whether the data concerned are sacred or non-sacred. The analysis of behavior patterns which involve high traditional values (like religion, family and sex, ethnic and group loyalties) should, of course, be as objective as possible, but an effort should be made to avoid needless and callous affront to the people who hold such values and such an effort requires special attention and care. In non-sacred areas (such as economic and political activities, housing and recreational problem) there can be greater freedom of reporting.

A related and equally important distinction must be made between public and private facts. This is something more than the difference between hidden and open knowledge. By definition, the sociologist deals with social and group relations. Hence, in a sense, his data can rarely concern completely private and secret activities. Nevertheless it is obvious that widely-known facts allow a

much wider margin of expression in the research report.

The manner of presenting the data may be equally important as the kind of data presented in increasing or lowering the intensity of the moral problem of what to report in a community or small group study. Although the custom of sociologists of providing anonymity to the community, group, or individual is not an adequate safeguard of the rights of these subjects, it does make possible a wider margin of expression than would a complete and open identification. There are, however, other and more important differences of mode of presentation.

The happiest situation for the social scientist is one in which statistical analysis of, and reporting on, the actions and characteristics of people is possible. Where large numbers of people are involved it is obvious that the problem of ethical limitations on the report hardly exists. But even in communities where situations are revealed that may be somewhat distasteful, the sub-groups and the individual may be adequately protected by the use of statistical categories.

As soon as the sociologist leaves the field of quantitative analysis and attempts to describe in conceptual terms the social relations in a small group or community, the problem of what to report becomes much greater. Even when the community is cloaked in anonymity, indirect identification is almost always possible, and there is likely to be a subtle and unintended violation of human rights. The threat becomes even greater when the sociologist adds to his description of the social relations in the group or community an interpretation of the motivation which supports these relations and other social behavior. Thus, where systematic sociological description and interpretation of motivation combine, the sociologist faces the gravest moral challenge, and particularly so where this mode of description and analysis is applied to a leading member of the group. The likelihood that such a person will be identified and his social behavior and personal reputation placed under scrutiny by his fellows on the basis of the research report is very great. Here, more than anywhere else, the sociologist must take care not to needlessly injure another human being.

The problem of truth telling thus becomes

a circumstantial one. This means that while telling the truth cannot *per se* be wrong or harmful, the ethical question of whether or not to include a certain objective fact always arises in relation to person and circumstances. Thus complete objectivity, or telling all the truth in all circumstances, is not necessarily a morally good act.

This is true for several reasons. The researcher is, of course, bound to secrecy where information has been given in confidence or where he has made promises of secrecy. At the same time, as a scientist, he will discover natural secrets, which by their seriousness demand silence on the part of the reporter. There is also the problem of detraction—the injury of another's reputation by revealing what is detrimental but true about him. If the harmful fact is already widely disseminated or if the subject is mistaken in the belief that the fact will result in the impairment of his reputation, the sociologist may not have any obligation to conceal the fact. Otherwise its revelation is a serious matter.

In summary, it can be said the problem appears in its most intense form when some member of a community or group is singled out for description and analysis and where such description and analysis may result in the revealing of secrets, the violation of privacy, or the detraction of reputation. Placed in this situation the sociologist must evaluate the claims of the individual, or of the sub-group and community, in their relations with the claims of the research sponsors, the donors of funds, the publisher of the report, the expectations of colleagues, and the rights of the larger society. We suggest that if the researcher accepts the values of human dignity and worth and does not want needlessly to injure the objects of his investigation, he will take the following four variables into account in attempting to arrive at a decision.

1. *The sociologist's definition of the nature of science.* Some positivists seem to regard science only as a fascinating game played according to a set of rules.<sup>1</sup> It is

<sup>1</sup> "Science after all is only one of the games played by the children of this world, and it may very well be that those who prefer other games are in their generation wiser." Carroll C. Pratt, *The Logic of Modern Psychology*, New York: The Macmillan Co., 1939, p. 57.



doubtful that the sociologist using this conception of science may ever legitimately overrule the rights of the people studied. The simple wish of the people to conceal certain aspects of their behavior must then be considered sufficient to bar the report of that behavior.

If one regards science as a search for truth as an end in itself, the demands of the objectivity of science will carry much weight in the decision to publish all pertinent data. Except in history, however, the truth for which the social scientist searches is nomothetic, not idiographic, truth. It may be necessary to base generalizations on certain idiographic items, but man has the entire span of his career on earth to discover and disclose such items. Certainly a particular item of current behavior turned up in a community study need not be used to support a generalization if such use inflicts injury on the people being investigated.

There is a third conception of pure science. Social scientists may believe that science is both a rigidly ruled game and a search for truth which is valuable for itself, but they usually also believe that science well developed and used by experts or disseminated among the people can make for a better life. There is a sense of urgency about accomplishing this mission of pure science in the modern world. Thus, within this perspective, considerable pressure arises to ignore the rights of people who are scientifically studied. Despite this pressure it remains true that a wilful disregard for the rights of persons and groups to their privacy, reputations, and secrets, will tend to destroy the very values which the scientist hopes his basic research can render more achievable.

Frequently the scientist makes a community or small group study not as a pure scientist but in one sense or another as an applied scientist. He may carry on the research for what he himself considers desirable practical ends; he may be employed by officials of the community or group or by those of the larger society; or he may be employed by some private group with a specific selfish or altruistic interest. In all three of these instances there is pressure to report all the significant findings even though injury may be done to the objects of the study. Nevertheless the sociologist

must abide by the rule that he exercise every effort to determine whether or not the values to be implemented by the study, and the probability of being able to achieve them through the use of its findings, justify the harm done to the members of the community or group.

Preoccupation with applied science is frequently accompanied by the temptation to look for and publish data which will further the realization of what the researcher himself regards as the good society or community. He is likely to believe that all of his data must be revealed in all circumstances. It appears to us that a scientist of this persuasion is most in need of the virtues of tolerance, compassion, and love, because he is in danger of placing the considerations of the "good" society above all consideration of individual rights and injuries.

The hired scientist, moreover, cannot avoid responsibility for revealing data injurious to individuals and groups by pleading loyalty to community or nation or by indicating his contractual responsibilities to a private group. Loyalty to community or nation may require injury to individuals and groups, but in such cases the scientist shares whatever guilt is incurred with all other responsible agencies. In instances of purely contractual research the scientist must accept full responsibility, because loyalty to nation or community is not involved. He is free to refuse the job, and if the values of the employing group are wrong or do not justify the amount of injury done the scientist must accept the moral responsibility.

2. *Determination of the extent to which a person or group will be injured by the publication of data concerning their behavior.* Those instances in which the scientist can foretell with certitude that serious injury will be done to the objects of his study seem to be very few in number. It is also likely that the largest proportion of his data will be free of possibly injurious materials. It is the in-between area of probable injury that is most difficult to determine and yet which must be determined.

To know what the effect of exposing a group's secrets will be, to realize how seriously a person's reputation may be damaged, and to visualize the effects of violation of privacy presupposes knowledge on the part

of the scientist which he may not have. This knowledge can be approached to the extent to which the scientist saturates himself in the social relations of the group which he studies. It probably cannot be achieved by the aloof scientist who simply culls the reports of those who have done the actual and basic data collecting.

Since there is a great difference between imaginary and objective derogation of reputation, the sociologist may tend to brush off the former as relevant and uncontrollable. Human decency, however, would seem to require that the scientist make an effort to inquire even into this possibility of psychological and subjective injury. The scientist cannot guard against all such contingencies and against the unexpected and unwarranted complaints of people, but he should do his human best to avoid them ahead of time and to be sympathetic to them if they come.

If the sociologist attempts to interpret the social behavior of the people he studies, he must assess the responsibility of the people for their own actions. False sentimentality must not result in the denial of the fact that a person must accept the consequences of the acts for which he is responsible. The scientist cannot erase the responsibilities, duties, and obligations, of the objects of his study. Yet, at the same time, he must recognize that the human being is never completely responsible for his actions, and that in many cases factors over which the person or group has no control may come close to completely determining certain acts. Since the assessment of responsibility will be contained in the research report, injury can be done if the assessment is not carefully made.

3. *The degree to which people or groups are actually members of a moral community of which the scientist is also a member.* At the core of the Western value system is a belief in the basic dignity and worth of the human being. This belief is based on different assumptions according to the particular stream of tradition in which one locates it: the Fatherhood of God, natural law, universal human needs and aspirations, or human reason. Whatever the base, the belief implies that men are bound to one another in a moral community. Membership in this community requires that the individual's rights to privacy, secrecy, and repu-

tation be respected, even though the human beings studied may not be members of the sociologist's own society.

The belief also implies that a man or group can renounce membership in the moral community by choosing modes of action which violate these basic values of dignity and worth. In mid-century it seems probable that men like Hitler and Stalin, organized groups like "Murder Incorporated," the Ku Klux Klan, and some others, have placed themselves outside the moral community and have surrendered the protection of its norms. Thus the social scientist need have no qualms about reporting in full detail the activities of such groups and people. Although this norm has never been explicitly formulated, it has guided a great deal of the research and reporting in social science.

Yet the decision of the sociologist to place particular persons or groups outside the moral community involves great responsibility, and he must be careful that his criteria of judgment permit tolerance, compassion, and wisdom. This is especially the case when he studies "unpopular" racial, religious and political groups, prostitutes, homosexuals, drug addicts, and the psychologically ill, the poor and powerless. It is hardly questionable that these people remain members of the moral community and hence retain their rights of privacy, respect, and secrecy. The needs of the society may require a limitation of their rights by the courts or by the social scientist in his reporting, but basic rights can be limited only to the extent that they *must* be limited. Beyond that point such people must be treated in the same way as other members of the moral community.

The recognition of basic human rights which accompany membership in the moral community is an important means by which social scientists can avoid the dangers of the use of purely subjective criteria. Within the consensus of the Western tradition it is objectively true that there are moral evils and modes of action which place the perpetrator outside this community. We must know as much as possible about such people and the scientist need have little inhibition in the report he provides about them. All other persons and groups, no matter how personally distasteful to the scientist, seem

to require the respect of their fellow-members in the moral community.

4. *The degree to which the larger society, the local community, or the group, needs the data of the research.* Real urgency must be defined in terms of the pressing needs of a group, community, or society, or in terms of some impending problem of which the scientist but not the group or community being studied is aware. Rights and duties are never unqualified in society and one of the qualifications seems to be that the society sometimes has a prior right to information which is necessary and useful for itself even though it may be harmful to an individual or sub-group.

The social scientist may find himself in one of several moral situations when he is trying to determine whether or not the social need is greater than the individual or group right. If the duly appointed authorities of a community or of the larger society believe certain information to be vitally needed, there is a *prima facie* case for the scientist to reveal such information. However, these authorities must show to the scientist the ground for the need. If he does not know and cannot find out from the authorities whether there is an urgent need for certain data which will be harmful to individuals and sub-groups, he is free of moral obligation to reveal it. If he is certain that the information is not necessary, he may in good conscience refuse to reveal it even though the authorities demand to know it. It must be recognized that his freedom in such instances is moral and not legal, and he may have to pay a price for his refusal.

In a similar manner the obligations which the scientist has to the group studied may require the revelation of information damaging to individuals or sub-groups. In this instance the scientist himself is likely to be the best judge of the need for his data. If he understands and accepts the basic values of the group and takes his obligation to the group seriously, he may find it imperative to disclose such information. Since he cannot

plead ignorance, and since there is no demand from competent higher authority, the responsibility for the assessment of urgency rests squarely on the scientist.

Finally, even though neither the higher authority nor the representatives of the group studied place any demands upon him, he may become aware of facts which are vitally needed by the social group studied or by the society. In such cases he must not only accept the responsibility for violating the rights of individuals and groups, but also must arrive at his decision with very little outside aid. In clear-cut instances where the comparison and balancing of the rights of the various claimants can be easily accomplished, the decision may be easily reached. But it is certainly in this area that the researcher will be forced to consider most thoroughly the importance which he, himself, has placed on the value of the information in its relation to the needs of the group.

The complexities exhibited in the discussion of the four central variables indicate that the problem of ethical limitations on sociological reporting cannot be reduced to a simple either-or proposition of a conflict between the scientific objectivity of a research report and the ethical inhibitions of the person who writes the report. It is apparent that the sociologist must act simultaneously according to a highly developed procedural code for scientific reporting and a code of ethics based on the belief that the objects of his study are also subjects. These codes are not irreconcilable, but the resolution of specific conflicts between them may be a very complex task, involving the claims of many groups and the interrelationships of the four variables. Yet the sociologist must resolve them. If there is a tendency for the sociologist to become more scientific, he must also become increasingly sensitized to the rights, feelings, and needs of the people he studies. Treating them as subjects means that to the best of his ability he will treat them with justice, understanding, compassion, and, in the last analysis, love.

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## IS A NEW FAMILY FORM EMERGING IN THE URBAN FRINGE?

E. GARTLY JACO and IVAN BELKNAP

University of Texas

ONE modern school of family sociologists, represented chiefly by Zimmerman, considers the present urban American family an alarming instance of disintegration in the familial process. This disintegration is believed to have reached such extremes that the family can no longer adequately discharge vital functions such as reproduction and socialization. Writers of this school imply that American, and Western civilization generally, faces the dilemma of social collapse through failure of the family functions, or a return to some form of the large rural or semi-rural family system.<sup>1</sup>

Another school considers the modern urban family to be making a reasonably satisfactory adjustment to population density, secondary relations, and diversity of urban institutions.<sup>2</sup> Most demographers apparently agree that basic population trends have been in harmony with the assumptions of this latter school.<sup>3</sup> Urban sociologists

also have generally accepted the present urban "companionship," or small family as necessarily typical of urban communities.<sup>4</sup> Burgess and Locke consider this type of family as one "which seeks to combine the values of both the old rural and the modern urban situations."<sup>5</sup>

It is the purpose of this paper to point out certain trends that may be leading toward the emergence of a variant type of urban family which may be able to maintain sufficient fertility and integration to satisfy the Zimmerman requisites and yet function adequately in the urban community. This variant type of urban family seems to be locating in the urban "fringe,"<sup>6</sup> as a product of changing ecological and demographic forces in metropolitan regions, and as a new functional adjustment of

*Journal of American Statistical Association*, 45 (September, 1950), pp. 335-349. C. V. Kiser's review of P. K. Whelpton's *Cohort Fertility* supports this contention in general, though less emphatically. See "Fertility Trends and Differentials in the United States," *Journal of American Statistical Association*, 47 (March, 1952), pp. 25-48.

<sup>4</sup> See L. Wirth, "Urbanism as a Way of Life," *American Journal of Sociology*, 44 (July, 1938), pp. 1-24; N. Anderson and E. C. Lindeman, *Urban Sociology*, New York: Knopf, 1928; B. A. McClenahan, *The Changing Urban Neighborhood*, Los Angeles: University of Southern California Press, 1929; E. R. Mowrer, *Family Disorganization*, Chicago: University of Chicago Press, 1927; W. F. Ogburn, *Social Characteristics of Cities*, Chicago: 1937; S. A. Queen and L. P. Thomas, *The City*, New York: McGraw-Hill, 1939; C. F. Ware, *Greenwich Village*, Boston: Houghton-Mifflin, 1935; H. W. Zorbaugh, *The Gold Coast and The Slum*, Chicago: University of Chicago Press, 1929. This typical family is described as (1) small in size; (2) equalitarian in member relations; (3) individualistic in terms of family formation and functioning; (4) tending to be located in multiple-family dwellings; and (5) lacking many of the functions of the rural family.

<sup>5</sup> Burgess and Locke, *op. cit.*, p. 143.

<sup>6</sup> The fringe herein considered includes suburbs, satellite cities, and any other territory located immediately outside central cities whose labor force is engaged in non-farm activities.

<sup>1</sup> See C. C. Zimmerman, *Family and Civilization*, New York: Harper, 1947, who concludes that "unless some unforeseen renaissance occurs, the family system will continue headlong its present trend toward nihilism" (p. 808). Also E. Schmiedeler, *An Introductory Study of the Family*, New York: Appleton-Century-Crofts, 1947 (revised). The dilemma consists in the fact that in neither case can the present structure of social organization be maintained.

<sup>2</sup> E. W. Burgess and H. J. Locke, *The Family*, New York: American Book Company, 1945; and J. K. Folsom, *The Family and Democratic Society*, New York: John Wiley, 1943. For a discussion of both schools, see R. F. Winch, *The Modern Family*, New York: Henry Holt, 1952, pp. 472-474, and W. Waller and R. Hill, *The Family*, New York: Dryden Press, 1952, pp. 17-20.

<sup>3</sup> The recent jump in fertility and marriages is generally regarded as a temporary phenomenon, holding that as knowledge and practice of contraception and other effects of urban life continue to reach the high-fertility segments of U. S. population, the downward trend will be resumed. See F. W. Notestein, "The Facts of Life," *The Atlantic Monthly*, 177 (June, 1946), pp. 75-83, and his "The Population of the World in the Year 2000,"

the family to the urban way of life. Because this family apparently represents primarily an adjustment to or a product of the peripheral metropolitan ecological area, it might be tentatively termed the "fringe family."<sup>7</sup>

This new family form may be only temporarily connected, however, with the urban fringe. The new family type should not be construed as being permanently or intrinsically bound up with the "fringe." The "fringe family" label is offered only as a provisional, heuristic term which implies that this family form is initially a product of contemporary urban fringe development. This form may eventually spread to other ecological areas. Further research is needed, therefore, before a precise term pertaining to the social structure of this new family type can be given.

Satisfactory proof of the existence of such family form in significant numbers will require restudy of parts of current urban family sociology. In view of the present status of sociological data on the fringe, however, such proof will require considerable field research. Little attention has been paid by sociologists to any family form associated with the outskirts of the city.<sup>8</sup> Also little systematic research has been done on the fringe as a social unit. At present, census

<sup>7</sup> This is not to take issue with either school. The type of family Zimmerman holds to be basic we regard as functional in rural areas. The companionship type of family is associated with *central cities*. However, with the current growth of fringe areas around central cities, we feel that a new urban family form is developing within this fringe. If this is the case, it would be a serious oversimplification to continue to regard the "companionship" family as typical of the entire urban community. Some urban sociologists are suggesting that the urban family forms need restudy. See, for example, Svend Riemer, *The Modern City*, New York: Prentice-Hall, 1952, pp. 255-259.

<sup>8</sup> It has come to the authors' attention just before going to press that Cavan has also discussed the development of new family processes in the urban fringe. (See R. S. Cavan, *The American Family*, New York: Thomas Crowell Co., 1953, Ch. 4.) See also the discussion of the suburban matricentric family by E. R. Mowrer, *The Family*, Chicago: University of Chicago Press, 1932; and by Burgess and Locke, *op. cit.*, pp. 131-134. Also R. E. L. Faris, *Social Disorganization*, New York: Ronald Press, 1948, pp. 462-465; and G. A. Lundberg, M. Komarovsky, and M. McNery, *Leisure: A Suburban Study*, New York: Columbia University Press, 1934.

and vital statistics data are inadequate for a thorough statistical analysis of the fringe; the former offering only data for 1950 on Standard Metropolitan Areas (these will not be useful for comparisons until 1960); and the vital statistics have no fringe definition at all. The census category of "rural nonfarm" is for the present about the only index of the fringe, but it includes many non-fringe components.

#### TRENDS TOWARD THE FRINGE FAMILY

The following trends in American society do not prove conclusively the existence and operation of a fringe family. They do, however, indicate a very strong probability of the development of this form. Demographic, ecological, labor force, and stratification data justify the inference that such concerted forces *must* be affecting and sustained by the family system existing in such an area.

*Demographic.* Birth order is obviously an index of increasing or decreasing family size. The boom in first order of births during the war years has evidently not been sustained recently.<sup>9</sup> However, an analysis of higher birth orders between 1942 and 1949 (comparative percentages computed by dividing the number of each birth order by total live births) reveals some noteworthy changes (Regional Summaries appear in Table 1.<sup>10</sup>) Every state reporting has an increase in third order of births in 1949 over 1942. For fourth order of birth, 43 out of the 47 states reporting had equal or greater percentages for 1949 over 1942; the four states having less in 1949 were Arkansas, New Mexico, Tennessee, and West Virginia, the latter having the greatest disparity of only one per cent. Apparently the fourth order of birth was the peak increase in 1949 over 1942, since only 17 states had equal or higher percentages of fifth orders in 1949. For the sixth order of

<sup>9</sup> Kiser, *op. cit.*, p. 25, among others. Demographers have tended to concentrate fertility analysis to either first or second or "very high" birth orders, ignoring the middle range which is significant to our thesis. See, for example, F. W. Notestein, "The Population of the World in the Year 2000," pp. 337 ff.

<sup>10</sup> Massachusetts is omitted from the New England region because of an absence of birth order data.

birth, only nine states showed an increase in 1949. In sum, between 1942 and 1949, all states had higher third order of births in 1949, and 43 out of 47 for fourth order. Seventeen states showed a consistent gain in third through fifth orders in 1949 over 1942: Louisiana, Maryland, New Hampshire, Oregon, South Carolina, Texas, California, Connecticut, Idaho, Illinois, Michigan, Minnesota, New Jersey, New York, Ohio, Washington, and Wisconsin. The first six states were consistently higher in percentages from the third through the fifth birth orders. For the U. S. generally, Whelpton's cohort study substantiates our results in his 1925 cohort's experience.<sup>11</sup>

Scaff's study of a California suburb showed that the "commuting population adds young families and comparatively larger families to this community." Furthermore, "without question, the presence of the commuter group in the community introduces younger adults and children and helps to balance an age distribution that is otherwise heavily weighted by elderly people."<sup>14</sup>

While the census area of rural non-farm is not strictly identical to that of the urban fringe area, it does include the latter and offers a crude index of trends in the fringe. In 1949, for the first time, the number of children under 5 years<sup>15</sup> of age per 1,000 women was higher in the rural non-farm

TABLE 1. PERCENTAGES OF 3RD, 4TH, 5TH, AND 6TH BIRTH ORDERS IN U. S. BY REGIONS, 1942 AND 1949 \*

Region	Birth Order							
	Third		Fourth		Fifth		Sixth	
	1942	1949	1942	1949	1942	1949	1942	1949
New England	12.3	16.1	6.2	7.4	3.4	3.5	2.0	1.9
Middle Atlantic	12.0	15.1	5.9	6.6	3.2	3.1	1.9	1.6
East North Central	12.9	16.5	6.7	7.8	3.6	3.8	2.2	2.1
West North Central	13.7	16.8	7.6	8.4	4.5	4.3	2.8	2.4
South Atlantic	13.2	15.4	8.3	8.6	5.6	5.3	4.0	3.7
East South Central	13.3	14.8	9.0	8.9	6.3	5.9	4.7	4.2
West South Central	12.5	16.1	7.2	8.9	4.6	5.4	3.1	3.5
Mountain	14.4	17.2	8.3	9.1	5.2	4.9	3.3	2.9
Pacific	11.9	17.1	5.5	7.3	2.8	3.1	1.6	1.6

\* Compiled from *Vital Statistics of the United States*, U. S. Government Printing Office, Washington, D. C., Part II, 1942 and 1949.

Kiser has pointed out the increase in fertility ratios since 1940 which are

proportionately heaviest among groups previously characterized by lowest fertility. Thus the percentage increases have been larger in the Northeast than in the South, larger among whites than nonwhites, larger among urban than rural-farm populations, and probably larger in the "upper" than in the "lower" socio-economic classes.<sup>12</sup>

Further data of a demographic order are suggested by Firey's finding of an excess of children under 10 years of age in the fringe of Flint, Michigan, in 1945.<sup>13</sup>

area than in both urban and rural farm areas, showing a steadier and higher rise than for urban areas, while in 1949, the rural farm amount dropped.<sup>15</sup> Further, the per cent distribution for 4 persons per household in 1950 was highest for rural non-farm areas as compared to both urban and rural farm areas.<sup>16</sup>

A recent study indicates an increase both in intra-urban and inter-urban migration to fringe areas.<sup>17</sup> A sustained drop in rural-urban migration and in immigration points

<sup>11</sup> Kiser, *op. cit.*, Fig. 1, p. 30.

<sup>12</sup> *Ibid.*, p. 38.

<sup>13</sup> W. I. Firey, *Social Aspects to Land Use Planning in the Country-City Fringe*, Michigan State College Agricultural Experiment Station Special Bulletin No. 339, East Lansing, June, 1946, pp. 17-18.

<sup>14</sup> A. Scaff, "The Effect of Commuting on Participation in Community Organizations," *American Sociological Review*, 17 (April, 1952), p. 217.

<sup>15</sup> Bureau of the Census, *Statistical Abstract of the United States*, Washington, D. C., 1951, Table 24, p. 20.

<sup>16</sup> *Ibid.*, Table 33, p. 25.

<sup>17</sup> R. Freedman, *Recent Migration to Chicago*, Chicago: University of Chicago Press, 1950.



in the direction of even further growth for the fringe.

Thus, with a spurt in higher birth orders and excess numbers of young children associated with a jump in fringe in-migration, a trend toward increased family size in the fringe can be deduced.

*Ecological.* Perhaps the most significant single index of the increase in the fringe population is the comparative rates of growth between 1940 and 1950: central cities grew 13 per cent; the hinterland increased 5.7 per cent; but the outlying parts of central cities showed a jump of 34.7 per cent.<sup>18</sup>

Home ownership is directly related to large families, both in urban and rural areas.<sup>19</sup> Census figures show that home ownership has increased 53.9 per cent between 1940 and 1950,<sup>20</sup> a change which may stimulate or be stimulated by larger families. The rate of home ownership in rural non-farm areas from 1930 through 1950 has been closer to that of the rural farm than to that of the urban area.<sup>21</sup>

An increase in the processes of concentration of population and decentralization of services is mentioned by Hauser<sup>22</sup> and Blumenfield.<sup>23</sup> Therefore, the growth of the urban fringe need not represent a deconcentration of urban population from central cities, but may indicate rather an expansion of urban population into broader territory. Many fringe areas exist apart from central city political boundaries only for a brief time until they become incorporated into the central city. However, the data accumulating on the ecology of the urban community do indicate a continuing increase in the distribution of the United States population into the fringe areas.

*Labor Force.* Whetten and Mitchell's

study of a Connecticut suburb showed that "white-collar" workers predominated among occupational groups.<sup>24</sup> However, movement to the fringe is not confined to the middle and upper economic levels. The spread of the concept of the guaranteed annual wage in American industry, taken with the increase in fringe population, indicates a drift to the fringe and to single-family dwellings by the so-called "blue-collar" workers.<sup>25</sup> The bearing of the guaranteed wage on the possibility of home ownership in this latter group is clear enough. Moreover, by "living out" and "working in," as Liepmann puts it, the blue-collar worker may paradoxically increase his family stability as he increases his job mobility. By living in the fringe, that is, the worker ceases to be tied to a particular factory or combination residential-occupational area in the city. He is free to change his jobs without the disruption of family stability caused by residential relocation.<sup>26</sup>

With the increasing employment of women, single as well as married, many women who eventually marry and become mothers return to their early forms of work after their children reach a more independent age (Table 2). Increased employment of married women is conducive to fringe living. Being employed in an area distant from residence minimizes the conflict between the mother's familial and non-familial roles. It seems highly probable that the increase of employment of women in the higher age groups is partly an index of employment of fringe family mothers.<sup>27</sup>

Liepmann has pointed out that fringe families encourage and even require "secondary earners," particularly in lower eco-

<sup>18</sup> From P. K. Hatt and A. J. Reiss, Jr. (editors), *Reader in Urban Sociology*, Glencoe, Illinois, Free Press, 1951, p. 68.

<sup>19</sup> M. Parten and R. J. Reeves, "Size and Composition of American Families," *American Sociological Review*, 2 (October, 1937), p. 664.

<sup>20</sup> Bureau of the Census, *op. cit.*, Table 866, p. 723.

<sup>21</sup> *Ibid.*, same table; also Table 871, p. 725.

<sup>22</sup> P. M. Hauser, "The Changing Population Pattern of the Modern City," in Hatt and Reiss, *op. cit.*, pp. 165-182.

<sup>23</sup> H. Blumenfield, "On the Growth of Metropolitan Areas," *Social Forces*, 28 (October, 1949), pp. 59-64.

<sup>24</sup> N. L. Whetten and D. Mitchell, "Migration from a Connecticut Suburban Town, 1930-1937," *American Sociological Review*, 4 (April, 1939), pp. 173-179.

<sup>25</sup> For a discussion of this possibility, see A. J. Jaffe, "Population Trends and City Growth," in Hatt and Reiss, *op. cit.*, pp. 188-189.

<sup>26</sup> K. K. Liepmann, *The Journey to Work*, New York: Oxford University Press, 1944, pp. 10-12. Logically, this should apply to the white-collar worker as well.

<sup>27</sup> A. J. Jaffe and C. D. Stewart, *Manpower Resources and Utilization*, New York: Wiley, 1951, p. 133, show that the rate of married women in the labor force increases as the age of their children increases.

TABLE 2. PERCENTAGE OF WOMEN IN U. S. LABOR FORCE, BY MARITAL AND FAMILIAL STATUS, 1949 \*

Status	Per Cent in Labor Force
All women in labor force	22.5
Women without children	28.7
With children under 6	10.0
With children some 6-11 only	24.7
With children some 12-17 only	31.3

\* Adapted from A. J. Jaffe and C. D. Stewart, *Manpower Resources and Utilization*, New York: Wiley, 1951, Table 4, p. 133.

nomic groups.<sup>28</sup> There are equally cogent reasons for the employment of higher status fringe mothers in view of the increasing costs of educating children, intensified by the inflation which has persisted since World War II. Lower age at marriage makes such employment of mothers consistent with a higher birth rate, while their superior education permits them to seek employment affording an economic surplus after they have paid for maids, kindergartens, and other maternal surrogates for the youngest children.

*Social Stratification.* Scaff holds that "education and membership in a profession become a badge of acceptance" in suburbs.<sup>29</sup> Coupled with its matricentric orientation, social stratification in the fringe is probably more distinct and overt than is apparent in the central city, where the "elite" is composed of professionals and their wives or widows, while industrial workers occupy lower social strata. If so, then social cleavages in the fringe may be disparate, and fixed along occupational lines.

Furthermore, if industrial workers make up the lower social strata and participate less in the suburban community, as Scaff's study indicates, then commuting by such a population may be viewed as "escape

from status." That is, by "working in and living out," a worker may absent himself from his inferior social position during the working day. This would be especially true if the wife and older children are also employed. Indeed, such flexibility may make more tolerable the occupancy of a lower social position in the fringe.

#### THE STRUCTURE OF THE FRINGE FAMILY

If one follows the clues suggested by current urban population and economic trends in the light of what is already known about the modern suburban family a number of inferences on the structure of the fringe family becomes possible. Some of the more important of these inferences involve the probable fringe family roles and the integration of this family at various stages of the institutional life cycle.

When the family selects the fringe for the sake of rearing children, this selection can be regarded as involving emphasis on the reproductive-socializing roles of father and mother. There is evidence that some movements to suburbs are carried out to improve the educational and recreational life of children.<sup>30</sup> This improvement is a reciprocal affair, since in its very nature it involves not only a greater control over the children's environment but a strengthening of the significance of the parental roles and the associated roles of the siblings. This strengthening of parental roles has already been examined for the mother by Mowrer, and by Burgess and Locke in their studies of the matricentric suburban family.<sup>31</sup> With the increasing employment of mothers, the shorter work day and week, and the spread of relatively higher incomes among many employed classifications, the father will come to play a more prominent role in the family and the community than

<sup>30</sup> Arthur Jones found in a study of a Philadelphia suburb that 80 per cent of its families had moved there to give their children better educational and recreational opportunities. (*Cheltenham Township*, Philadelphia: University of Pennsylvania Press, 1940, pp. 51-52.)

Strictly speaking, the parental roles of mother and father represent the center of the family functions, rather than the husband-wife roles. The central-city companionship family stresses the latter; the fringe family the former.

<sup>31</sup> E. R. Mowrer, *The Family*, Chicago: University of Chicago Press, 1932; Burgess and Locke, *op. cit.*, pp. 131-134.

<sup>28</sup> In this process the family assures itself some safety through economic diversification, while at the same time it develops in what is probably an equalitarian direction. (Liepmann, *op. cit.*, pp. 19-25). Liep ann states further, "... The family as a whole benefits from the varied employment of its members. It is economically safer for the family not to have all of its eggs in one basket, i.e., not to depend on one industry which may decline while others prosper. Domestic life, moreover, is enriched by a variety of occupational interests among the family," p. 24.

<sup>29</sup> Scaff, *op. cit.*, p. 220.

was possible either in the companionship or suburban matricentric family. In general it seems likely that all the family roles in the fringe family may be enhanced by proximity of members, and by mutual functional significance.

The Burgess viewpoint of family sociology, with certain qualifications, holds that the companionship family is more in line with other urban social institutions. In view of the powerful stress required by this form on the intrinsic husband-wife relationship rather than on the father-mother bond, the companionship family may represent less of an adaptation than a negation of the family's important functions. There is an implication in this position that as the family "gives in" and loses its historical functions, it becomes better adjusted to the urban environment.<sup>32</sup> Hence the fringe type of family offers at least a compromise between familistic and companionship forms while maintaining at least an apparent emerging adaptation to the urban environment.

Implicit in the strengthening of the parental bonds in the fringe family is an increased control over the courtship process, and perhaps solidarity in the old age family roles.<sup>33</sup> In the central city, anonymity and diversity of interaction minimizes parental control over children in the realm of courtship and dating. In the fringe, the courtship process can be confined in some degree to peer groups selected by parents in tacit agreement with other parents of like status. This represents a compromise between parental mate choice for the children, and the theoretical free choice implied in the dating pattern of the urban youth culture. This actuarial kind of control over the courtship process by fringe parents obviously gives greater continuity to the family process as experienced by both parents and children. This continuity, together with the heightened significance of the member re-

lationships, may be one explanation of the greater number of children in the fringe.

#### SUMMARY AND DISCUSSION

Burgess and Locke list six long-time family-related trends which have been disrupted by the recent war and speculate about their continuation after the war. These are: (1) The declining birth rate; (2) The consequent smaller size of the family; (3) The increase in proportion of the married to those of marriageable age; (4) The decrease in the age at marriage; (5) The increase in the proportion of all women, and of married women gainfully employed; (6) The decline in the historic functions of the family—economic, educational, recreational, religious, and protective.<sup>34</sup> The apparent assumption is that, should these trends be only temporarily disrupted by the recent war, and should they continue as in the past, the companionship family will become institutionalized in the United States.

Trends pointed out in the preceding discussion, however, indicate that particularly for the rapidly increasing U. S. fringe area, a somewhat different picture is appearing: (1) Sustained fertility through higher orders of birth; (2) A consequent increase in the size of the fringe family; (3) Marriage rates for males higher in the rural non-farm areas than in both urban and rural farm areas; (4) Decrease in the age at marriage continuing; (5) Employment of both single and married women increasing, particularly for the higher age groups and with mothers of children from 12 to 17 years of age; (6) The historic functions of the family seemingly better retained in the fringe—the economic, with employment of mothers as secondary workers; the educational, in the selection of "better" schools for children; the recreational, in the encouragement of participation of children in selected peer groups and social sets; the religious, in belonging to and supporting the "right" churches, and the protective, in addition to the preceding, in providing the best care and rearing practices of medical and mental science.

There is some indication, therefore, that the interruption of the long-term trends listed above by Burgess and Locke may

<sup>32</sup> See Zimmerman, *op. cit.*, Ch. 2.

<sup>33</sup> It seems very likely that the extension of one's family of procreation into some supervision and control of his children's courtship process may give greater integration to his family of old age (gerontation). This would be an important development in view of the present trend toward an aging population, and the present isolation of grandparents in the terminal segment of the urban family life cycle.

<sup>34</sup> Burgess and Locke, *op. cit.*, p. 750.



have become sustained in the fringe family. We can say definitely, at least, that this interruption has been associated with an enormous increase in the fringe population between 1940 and 1950, and that the concept of the fringe family may serve sociology as one useful research hypothesis for the analysis, in a relatively unexplored area, of the demographic, ecological, community structure and working force trends we have mentioned.

If a family form of the type suggested here is beginning to appear in the urban fringe, several current postulates in the social-psychology of personality development will also have to be reconsidered.<sup>35</sup> To urban sociological study, the presence and operation of this family will mean new possibilities and problems if the present trends continue toward fringe expansion of population, decentralization and subsequent relocation of industry and services in the fringe.

For the determination of the structure and function of the fringe family and the community processes associated with it, nine hypotheses are here suggested for further research:<sup>36</sup>

(1) More Protestant than Catholic or Jewish families appear to live in the fringe. If so, Protestant fertility may be rising, Catholic fertility declining, and Jewish fertility destined to continue at a low rate.

(2) More whites than non-whites live in the fringe. If this is true, white fertility may rise, Negro fertility decline even further.

(3) Social stratification may be more fixed and disparate in the fringe than in the central city. Analysis of stratification in the fringe should show new class criteria, and should suggest answers to such questions as whether social classes are be-

coming more or less numerous and rigid in the United States.

(4) The inhabitants of fringe areas are experimenting with new forms of age and sex social organization. New perspectives on the urban cultural life cycle for childhood, youth, maturity and old age for both sexes should appear in systematic studies of the fringe.

(5) In the fringe the kinship system is assuming a more prominent function as the basis of status. Is the strengthening of kinship in this sense making the family as important as occupation in determining status in the urban community? If occupation is still maintaining major importance as a status-basis in the fringe, is the kinship system becoming more important in maintaining occupational lines?

(6) A strengthening of sibling as well as parental family roles occurs in the fringe family compared to the central city family. This changed significance of the sibling roles should extend the range of kinship association among age peers and modify current urban voluntary association practices.

(7) When contrasted to the central city, the fringe presents the following differential demography: higher fertility, larger families, more marriage, more children, greater number in the labor force, more home ownership, greater fluidity, less mobility, lower mortality, more aged persons, lower age at marriage.

(8) There is a higher rate of family participation in social institutions in the fringe than in the central city. Re-alignment of the family with urban institutions has tremendous significance for the study of contemporary social organization.

(9) More parental control of marriage occurs in the fringe than in the central city. The influence of parents on courtship in the fringe may be such as to bring about new types of mate selection and courtship patterns differing from those associated with the urban companionship family.

The foregoing propositions are not meant to be exhaustive. It seems certain, however, in view of the evidence now at hand, that the verification of a few of these hypotheses should begin a new chapter in urban family sociology. Moreover, this verification will have decided implications for demography, ecology, and social psychology.

<sup>35</sup> Particularly those theories centering on the consequences to the developing child of membership in a family unit which is relatively small in numbers, feeble in social extent and power, and self-centered. See for a sociological treatment of these theories, Bossard, J. H. S., *The Sociology of Child Development*, New York: Harper, 1948, Chapter III.

<sup>36</sup> Each of these hypotheses may have corollaries which can be deduced in the process of setting up the research. Each of them must also be comparatively primitive in view of the current status of data on the fringe.

## PREDICTING MARITAL SUCCESS OR FAILURE IN AN URBAN POPULATION

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A FAIRLY recent development in family research has been the attempt to predict marital success or failure by utilizing premarital characteristics of the spouses. That predictive systems based on premarital characteristics can be useful cannot be denied. But since most of the marriage prediction studies have been essentially exploratory in nature, validity of prediction scales based on the studies has not been firmly established.

The studies involving measurement of marital success or of factors correlated with it have been criticized on various grounds.<sup>1</sup> The lack of adequate sampling procedures is perhaps the most glaring methodological weakness, and while this is excusable in pilot studies, the time for more adequate sampling procedures has now arrived.

The present study involved a check on the validity of the Burgess-Cottrell prediction scale when applied to a sample differing in composition from that used in its original development. Burgess and Cottrell distributed about seven thousand "anonymous" questionnaires, largely through the help of University of Chicago students. That the sample was selective in nature is indicated

by Burgess and Cottrell in the following statement: "A review of the characteristics of the sample used in the study points to the general conclusion that it is a roughly homogeneous, young, preponderantly non-neurotic middle class, native white American urban group."<sup>2</sup>

### THE SAMPLE

How could a more representative sample than was used in the Burgess-Cottrell study be obtained? Various sampling procedures were considered but eventually it was decided to use a random sampling method, whereby, with the names of a population arranged in order, cases are chosen at constant intervals, the specific interval depending on the ratio of the size of the sample to the size of the group from which it is drawn.

The author, partly as a matter of convenience, chose Akron, Ohio, as the area from which to draw the sample. Akron is an industrial city of a little less than three hundred thousand population. A city directory, listing the names of residents sixteen years of age or older and their marital status (in most cases) was available.

A sample of three hundred couples was selected from this directory. If an individual could not be found, or did not meet the requirements of eligibility for the sample, he was replaced by the person whose name followed his in the directory. The requirements for eligibility were: (1) The couples must be legal residents of Akron. (2) They must have been married in the United States. (3) Both members of the couple must still be living at the time of the interview.<sup>3</sup> (4) The individuals must be able to

<sup>2</sup> Ernest Burgess and Leonard Cottrell, *Predicting Success or Failure in Marriage*, New York: Prentice-Hall, 1939, p. 29.

<sup>3</sup> This requirement was based on the assumption that too great a bias might be prevalent in responses of a widowed person to questions concerning the deceased spouse.

<sup>1</sup> The most critical technical discussion of the marriage prediction studies has been made by Albert Ellis. See Albert Ellis, "The Value of Marriage Prediction Tests," *American Sociological Review*, XIII (December, 1948), pp. 710-718. Terman and Wallin in a later article ably answered Ellis. See Lewis Terman and Paul Wallin, "The Validity of Marriage Prediction and Marital Adjustment Tests," *American Sociological Review*, XIV (August, 1949), pp. 497-505. For perhaps the most thorough analysis of the marriage prediction studies see the chapter on Marital Success in Reuben Hill, *The Family*, New York: Dryden Press, 1951, pp. 343-374; and Clifford Kirkpatrick, *What Science Says About Happiness in Marriage*, Minneapolis: Burgess, 1947. See also William Kolb, "Sociologically Established Family Norms and Democratic Values," *Social Forces*, XXVI (May, 1948), pp. 451-456, for an analysis of marriage studies with regard to their value premises and implications.

understand English. (5) The couples must have been married a year or more.

Of the original 300 names chosen from the city directory 62.3 per cent were eligible for inclusion and cooperated fully, 23.3 per cent refused to cooperate, and the remaining 14.4 per cent could not be located or were ineligible for inclusion. New couples were chosen when members of the original 300 could not be found, were ineligible for inclusion in the sample, or refused to cooperate.

It was to be expected that the sampling procedure used would result in a heterogeneous sample. An examination of some of the major socio-cultural characteristics of the individuals from whom data were obtained leads to the conclusion that the sample was much more heterogeneous than those used in previous studies (with perhaps the exception of that of Harvey Locke).<sup>4</sup> This is especially true in regard to social class position of the sample members. Over half of the men in the sample were in the unskilled or semi-skilled labor categories, and 47 per cent had annual incomes of less than 1800 dollars per year (at the time of marriage). Thirty-two per cent of the husbands had no more than a grade school education when they married, and an additional forty-five per cent had terminated their education after one to four years of high school. One third of the women in the sample married before they had reached their twentieth birthday. Fifty-five per cent of them were in the unskilled or semi-skilled occupational categories at the time of their marriage (including domestic service). Twenty-six per cent of the women in our sample had grade-school education only and an additional fifty-one per cent had some high school education but no further formal education.

The sample was found to be biased toward Protestants of North European origin. This can be accounted for partly by the fact that Akron contains a large number of people who migrated from the Appalachian area of the South or whose ancestors did so. However, the sample included eight per

cent Negroes, about the proportion to be expected in a random sample of Akron.<sup>5</sup>

It is not claimed that the sample is completely representative of all sub-groups of Akron. However, the sampling procedure was designed to obtain a random sample of a population. Recognizing that its composition may be biased somewhat by refusals and by other factors, nevertheless it does provide a closer approximation to an urban random sample than does any previous study in this field.<sup>6</sup>

Preliminary contact letters were sent out, and these were followed by personal interviews. An effort was made to follow up the letters within three to seven days, whenever possible. The couples were interviewed personally by the author. In the interview the purpose of the study was explained, and the individual was assured that his responses would be used confidentially. The various ways in which the questionnaire could be filled out were explained. One of the simplest methods was for the interviewer to ask the questions and check off the verbal responses. If the individual preferred to check the answers himself, he was allowed to do so. In either instance the interview consisted of one "sitting," with the author retaining possession of the completed questionnaire. If the individual seemed apprehensive about filling out the questionnaire or having it used in an interview, he was given the alternative of completing the questionnaire and having it ready to be picked up at a later date at his convenience. Another alternative presented was a self-addressed envelope in which the questionnaire could be returned.<sup>7</sup> The author went to extreme lengths to as-

<sup>5</sup> See Atlee La Vere Stroup, "A Study of the Burgess-Cottrell System of Predicting Marital Success or Failure" (unpublished Ph.D. Dissertation), the Ohio State University, 1950, for a detailed discussion of the study on which this paper is based.

<sup>6</sup> No systematic study of the potential sample members who refused to cooperate was attempted. It is fairly certain that they were not a homogeneous group either in terms of class position or level of marital adjustment, although a number of them very probably were in the poorly adjusted categories. Theoretically the results of the study should not be significantly affected by having a non-cooperating group of this size.

<sup>7</sup> Where questionnaires were to be returned by mail, a code system was used whereby account could be kept of all questionnaires not returned.

<sup>4</sup> Harvey Locke, *Predicting Adjustment in Marriage: A Comparison of a Divorced and a Happily Married Group*, New York: Henry Holt, 1951, Chapter 2.



sure individuals that the information wanted would not in any way be used against them.<sup>8</sup>

#### HYPOTHESES

The questionnaire used contained items from the Burgess-Cottrell marital adjustment scale and background items from which Burgess and Cottrell had made their prediction scale. It was held that a positive correlation might be expected for the sample between the scores on the Burgess-Cottrell adjustment scale and the prediction scale, but that an obtained coefficient of correlation would be lower than it might be because the weights for the prediction items would not be quite correct.

Stated more formally, the major hypothesis was: If the Burgess-Cottrell marriage adjustment scale and marriage prediction scale are administered to a random sample of a new urban population: (a) and if the original Burgess-Cottrell weights for the marriage adjustment items and the marriage prediction items are used, a positive correlation will be found between the scores on the two scales; (b) but, if new prediction weights (obtained by the Burgess-Cottrell method of assigning weights) are used for the new sample, a significantly higher correlation will be found.

A second hypothesis was involved in the study. Burgess and Cottrell confined their sample to couples married from one to six years. They assumed that background factors would not be as closely associated with marital adjustment in the later years of marriage as they would be in the early years. The validity of this assumption was challenged and consequently persons were included in the Akron sample if they had been married for any length of time over a year.<sup>9</sup> The minor hypothesis was: No significant difference will be found between the correlation coefficient (for the adjustment and prediction scores) of those married one to six years and those married longer. This

<sup>8</sup> Emphasis was given to the establishment of rapport with the sample members in order to get as low a refusal rate as possible and to get valid responses to those test items of an intimate and personal nature. If resistance was offered to filling out the questionnaire during the first interview the alternatives mentioned above were discussed.

<sup>9</sup> The mean number of years married for the sample was 15.2.

was based on the assumption that background factors have a relatively permanent effect, whatever it is, on marital adjustment.

#### ANALYSIS OF DATA

Responses to the questionnaire items were scored and analyzed. The scores on the adjustment scale ranged from 22 to 194 with a mean adjustment score of 153.6. The background items were then analyzed to determine their association with the scores on the adjustment scale. The method used was that devised by Burgess and Cottrell. The items were arranged into tables and grouped in terms of their association with the "good," "fair," and "poor" ranges of the marital adjustment scores. New weights were assigned the items, these being determined by their discriminatory value in the Akron Sample.<sup>10</sup> Prediction scores were also obtained for each couple by the use of the original Burgess-Cottrell weights, those based on the discriminatory value of the items in the Burgess-Cottrell sample, so that two prediction scores were calculated for each couple.

With prediction scores available for each couple using both the new weights and the Burgess-Cottrell weights for the background items, it was possible to test our major hypothesis. A positive coefficient of correlation of .36 was obtained between the marital adjustment and prediction scores, using the Burgess-Cottrell original prediction weights. When the new prediction weights were used, a positive coefficient of .58 was found. The difference between the coefficients was found to be statistically significant ( $T=4.00$ ). Thus the major hypothesis is supported by the data.

To test the second hypothesis the sample was divided, arbitrarily, into categories of couples married one to six years, seven to fifteen years, sixteen to twenty-five years, and twenty-six years to sixty-one years respectively. Correlation coefficients were calculated between the adjustment and prediction scores for each of these periods by using both the Burgess-Cottrell prediction weights and the revised weights.<sup>11</sup> When the

<sup>10</sup> See Burgess and Cottrell, *op. cit.*, Chap. 14 for a detailed discussion of weighting system.

<sup>11</sup> The N's for the sub-categories on which the correlations were based were as follows: 84 for

original weights were used, the resulting coefficients (all positive) were (1) .38 for those married one to six years, (2) .32 for those married seven to fifteen years, (3) .35 for those married sixteen to twenty-five years, and (4) .36 for those married twenty-six to sixty-one years. By the use of the revised weights for the predictive items the following positive coefficients of correlation were found: (1) .56 for those married one to six years, (2) .67 for those married seven to fifteen years, (3) .57 for those married sixteen to twenty-five years, (4) .56 for those married twenty-six to sixty-one years.

In comparing the coefficients of correlation for the one to six years period with the other periods referred to above (using the Burgess-Cottrell original prediction weights), it was found that by the T test none of the differences were significant. The same held true when the revised prediction weights were used for the predictive items. The second hypothesis, therefore, seems to be supported.

Space limitations forbid a comparison of the findings on specific premarital background factors with those of Burgess and Cottrell. On a number of items a close similarity prevailed while on others some rather interesting differences appeared.<sup>12</sup> It is hoped that another article can be published dealing with a comparison of the Akron data with that of Burgess and Cottrell on the importance of the individual background items for the prediction scale.

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those married one to six years, 98 for those married seven to fifteen years, 43 for those married sixteen to twenty-five years, and 71 for those married twenty-six years or more. There were 4 cases in the "no reply" category.

<sup>12</sup> It is possible that differences in the relationship between selected background factors and marital adjustment found between the two studies reflect regional differences, but it is doubtful if all the variations could be so explained. It is possible also that the obtained variations in responses are random sampling errors which can be substantial unless large samples, involving many hundreds of cases, are used. It is recognized that when prediction weights are based on a criterion with which these weights are correlated, the two sets of scores are not independent and a somewhat spuriously high correlation is to be expected. This is probably a partial explanation for the increase in the coefficient of correlation found in this study. It is possible that the greater randomness of our sample is partly responsible, also.

#### INTERPRETATION

Since a positive coefficient of .36 was obtained between the two scales when the original weights were used on our sample, and a somewhat higher coefficient of .58 was found when revised prediction weights from our own sample were used, it would appear that the Burgess-Cottrell method of predicting marital success has some validity. If another sample were taken at random from the city of Akron, Ohio, the probability is that the revised prediction weights would be more correct than the original weights of Burgess and Cottrell. Nevertheless it will be rather surprising to some to find that as high a coefficient of correlation as .36 could be obtained on a sample selected in a quite different manner from that used in the development of the original scales and in a region and type of city different from Chicago, Illinois. Our findings are in line with another recent study of the validity of the Burgess-Cottrell method of measuring and predicting marital adjustment, the sample population involving an apparent cross section of the Negro couples of Greensboro, North Carolina.<sup>13</sup> In this study Charles King also found that a similarity existed in certain instances between his and the Burgess-Cottrell findings on the relationship between premarital background factors and marital adjustment, whereas on certain factors a divergence in findings appeared. When the background factors were combined into a prediction scale and husbands' and wives' background scores were correlated with their respective adjustment scores, a positive correlation of .49 was obtained. Since an  $r$  of .51 was obtained by Burgess and Cottrell, and in Akron an  $r$  of .58 was obtained, one could say that relatively similar coefficients of correlation are being obtained.<sup>14</sup>

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<sup>13</sup> Charles E. King, "The Burgess and Cottrell Method of Measuring Marital Adjustment Applied to a Non-White Southern Urban Population," *Marriage and Family Living*, XIV (November, 1952), pp. 280-285.

<sup>14</sup> In this study no attempt was made to devise new weights for the marital adjustment scale, nor were the responses of both spouses obtained. Rather it was assumed that the original weights were valid and the responses of one of the spouses only were elicited for the marital adjustment part of the questionnaire.

It seems plausible to suggest on the basis of this study that the effect of premarital background factors on marital adjustment is relatively permanent, although not necessarily equal at all stages of married life. Again further research is necessary. Important implications are involved here for theories of personality such as those regarding rigidity of personality traits.

We have assumed in this study that the Burgess-Cottrell Marital Adjustment Scale is a valid measure of marital success. It is here that the most critical theoretical problem facing all scientists making studies of marital success appears. What is marital success and how does one measure it? The Burgess-Cottrell Marital Adjustment Scale was devised by correlating certain items assumed to be at the core of marital adjustment with self ratings on a five point happiness scale. To a certain extent the validity of the scale is a matter of how valid are self-ratings of happiness. A completely satisfactory measure of marital success still remains to be devised.<sup>15</sup> Once this is accom-

<sup>15</sup> Burgess and Cottrell did find fairly high

agreement between self ratings and ratings by outsiders who were well acquainted with the couples. Locke, with a sample quite different in social background from that used by Burgess and Cottrell found that the scale clearly differentiated between divorced people and those reported by close friends to be happily married. This could indicate that the scale has considerable validity. The study by King seemed to indicate that the scale has some validity. It is possible, however, that the responses to certain items are biased in the direction of adjustment. Indirect measures of marital success such as Kirkpatrick's "Community of Interest" scale may be of value in this connection.

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## NOTES ON RESEARCH AND TEACHING



### PATTERNS OF FAMILY LIMITATION IN A RURAL NEGRO COMMUNITY

CHRISTOPHER TIETZE and SARAH LEWIT

The present study is based on the records of the reproductive histories of 357 Negro women in a rural area on the Eastern Shore of Maryland. The histories were obtained in 1950 and 1951 in connection with a field test of the acceptability and effectiveness of a simple contraceptive technique among a population whose previous history indicated that birth control had been practiced infrequently and with little success.

The survey was conducted in three election districts in Worcester County, Maryland, including one small town, Snow Hill. Truck farming was the basic industry of the area. There were also a number of saw mills and lumber yards, and a large frozen-food plant in a near-by community which offered additional opportunities for employment.

Under the supervision of one of the two physicians in the town, a trained social worker, Mrs. Irma H. Geddis, visited all Negro families in the survey area. A history was obtained from each married woman below the age of 50 years, including legally married women and those living in informal unions. The fact that the interviewer was the wife of a local colored clergyman, and was well known and respected in the area, doubtless facilitated the establishment of good rapport with the respondents.

In addition to personal, economic, and social data, the records contained a list of pregnancies for each woman, the date of termination of each pregnancy, its outcome, and information on the nature and duration of previous contraceptive practices, if any. The report is based on the histories obtained at the initial interview from all women, including those who later participated in the test of the contraceptive method and those who did not.

One-half of the 357 women in the survey population lived on farms. Ninety per cent reported their husbands as farmers, laborers, or semi-skilled workers of various kinds. Twenty-two husbands were described as craftsmen—mostly in the building trades—and four oper-

ated their own businesses. Five husbands were listed as teachers or clergymen.

The median level of formal education attained by the women in the Snow Hill survey amounted to 7.8 completed years of school. There was a fairly regular increase in number of school years with decreasing age of woman. The range was from 5.6 completed years of school for women 40 years of age and over, to 9.3 years for those under 20 years of age.

A total of 1,356 pregnancies was reported by the 357 women in the survey population, representing an average of 3.8 pregnancies per woman. The total number of live births was 1,198, averaging 3.4 per woman interviewed. Women born before 1910, that is 40 years of age and over at the time of the survey, had experienced an average of 5.4 live births. This figure can be compared with 7.0 live births for rural Negro women of the same age group in the South, according to the Census of 1910, and 4.6 live births for the corresponding group in 1940.<sup>1</sup> About one-sixth of these older women had never been pregnant while one out of four had experienced ten or more pregnancies. Of all pregnancies for the entire group, 87.5 per cent had produced one or more live babies, 3.6 per cent had ended in stillbirth, and 8.9 per cent in abortion. According to the respondents, none of the abortions was intentional.

Of the 357 couples in the Snow Hill survey, 67 husbands or wives, or 19 per cent of the total, were using a contraceptive method at the time of the interview or had used birth control just prior to a pregnancy still in progress. This proportion compares with 38 per cent in a group of white farm residents in the mountains of North Carolina,<sup>2</sup> about 55 per cent in Great Britain in the early 1940's, according to the Royal Commission's report on family limitation,<sup>3</sup> and an estimated 85 per cent in the

<sup>1</sup> U. S. Bureau of the Census, 16th Census, 1940. *Population; Differential Fertility 1940 and 1910; Women by Number of Children Ever Born*. Washington, D. C., Government Printing Office, 1945.

<sup>2</sup> Christopher Tietze and Clarence J. Gamble, "The Condom as a Contraceptive Method in Public Health Work." *Human Fertility*, 9 (December, 1944), pp. 97-111.

<sup>3</sup> United Kingdom Royal Commission on Population, "Report on an Enquiry into Family

group covered by the Indianapolis Study.<sup>4</sup> In addition, 34 women, or almost 10 per cent, had been sterilized by surgical operation. The only available comparative figure appears to be 8.5 per cent in Hatt's report on Puerto Rico,<sup>5</sup> computed on the basis of women married in 1920 or later.

Analysis by year of birth reveals that the younger women had practiced contraception earlier in life than had their older sisters. Thus, of 96 women born in 1905-14, only 7 per cent had used birth control before age 25; of 127 women born in the following decade, 1915-24, about 18 per cent; and of those born in the late nineteen-twenties, at least one-third. This finding illustrates the manner in which the contraceptive habit was spreading in the community.

Of the 67 couples using contraception at the time of the interview, 39 used condoms and 5 the diaphragm-and-jelly. Twelve couples relied on douches, 6 on coitus interruptus, and 8 on a variety of other methods. Because a few couples employed more than one method of birth control, the total number of methods used was greater than the number of couples using them.

The outstanding facts are the high incidence of douching and the low incidence of withdrawal, a pattern which seems to be peculiar to the American Negro. This finding is consistent with the observations of earlier investigators.<sup>6</sup> We cannot offer any explanation for this pattern of preference, but we feel that its existence has been satisfactorily established.

In general the survey group had not practiced contraception with a high degree of per-

severance and effectiveness. Beside the 67 couples using birth control at the time of the interview, there were 28 others who had done so at one time or another but had stopped after one or more failures. Some of these women were later sterilized.

Irregular use and lack of skill are indicated by a rate of 44 pregnancies per 100 years of exposure with contraception in Snow Hill, as compared with a rate of 12 among the "relatively fecund" couples in Indianapolis, and a rate of approximately 7 per 100 years of exposure in Great Britain around 1945. In only one or two instances in the Snow Hill survey was birth control discontinued in order to have a baby.

The lack of success with conventional contraception probably explains the high incidence of surgical sterilization, which had not been foreseen by the investigators when the study was planned. All operations had been performed immediately after delivery in a nearby general hospital administered by a non-sectarian, non-profit corporation. It was reported that the local Medical Association considered a sixth pregnancy an indication for sterilization and that this was generally known in the community. In fact, some women who had experienced pregnancies in rapid succession were said to be looking forward to their sixth baby which would make them eligible for the operation.

A few of the 34 sterilized women in the survey had undergone the operation after their fourth or fifth pregnancy because of some special medical indication while others had waited far beyond the sixth. On the average, the sterilized women reported 8.2 pregnancies and 7.4 live births. Both the mean and median age at the time of the operation were 33 years.

The incidence of surgical sterilization in this group has been referred to as high but this is no more than an impression. No information is available to what extent sterilization of women and perhaps of men is used by various groups in the United States as a method of family limitation.

#### A NOTE ON INCONSISTENCY IN PAIRED COMPARISON JUDGMENTS

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The method of paired comparisons is used to obtain a set of relative scale values for a given set of objects. The set of objects, arranged in all possible pairs, is presented to a panel of judges. Each member of the panel

Limitation and Its Influence on Human Fertility during the Past Fifty Years." By E. Lewis-Fanning. *Papers of the Royal Commission on Population*, Volume I, London, 1949.

<sup>4</sup> P. K. Whelpton and Clyde V. Kiser, "Social and Psychological Factors Affecting Fertility. VI. The Planning of Fertility." *Milbank Memorial Fund Quarterly*, 25 (January, 1947), pp. 63-111.

<sup>5</sup> Paul K. Hatt, *Backgrounds of Human Fertility in Puerto Rico*. Princeton: Princeton University Press, 1952.

<sup>6</sup> Gilbert W. Beebe, *Contraception and Fertility in the Southern Appalachians*. Baltimore: The Williams and Wilkins Co., 1942.

Raymond Pearl, "Contraception and Fertility in 4945 Married Women. A Second Report on a Study of Family Limitation." *Human Biology*, 6 (May, 1934), pp. 355-401.

Regina K. Stix, "Contraceptive Service in Three Areas." *Milbank Memorial Fund Quarterly*, 25 (January, 1947), pp. 171-188.

Christopher Tietze and Clarence J. Gamble, "A Field Study of Contraceptive Suppositories." *Human Fertility*, 13 (June, 1948), pp. 33-36.

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records his preference for one member of each of the possible pairs. Inconsistencies of judgment occur when A is preferred to B, B preferred to C, and C preferred to A. Such inconsistencies, here called *circular triads*, are to be considered as errors of judgment. As errors, inconsistencies decrease the reliability of the scale values computed from the comparative judgments of the objects.

If the rationale underlying the method of paired comparison scale construction is valid, then the occurrence of inconsistent judgments of objects should increase as the difference between those objects on the underlying continuum decreases. In other words, the greater the difference between objects with respect to the attribute being judged, the less likely these objects are to be judged inconsistently. The data reported below provide one test of this logical extension of Thurston's theoretical discussion of comparative judgments.<sup>1</sup>

A second problem to be discussed is that of individual differences in the ability to make the type of discriminations called for in paired comparisons. If there is some general ability concerned in the making of consistent discriminations, then individuals making inconsistent judgments in one situation would tend to make similar inconsistencies in a second situation.

#### METHOD

Three sets of objects to be judged by paired comparisons were submitted to two experimental groups. Two of these sets were composed of attitude statements concerning the participation of the United States in the Korean conflict. The third set of objects consisted of the names of nine occupations having professional status.

The attitude items had been scaled previously by the method of equal appearing intervals. On the basis of these scale values, a set of nine attitude items was selected so that the entire attitude continuum, from highly unfavorable to highly favorable, was represented. A second set of seven attitude items was selected to represent only the favorable portion of the attitude continuum.

Set I of the attitude items (containing the nine statements) and the set of occupational titles were presented to the first experimental group ( $N = 78$ ). The attitude items were judged concerning the degree to which they indicated favorability toward U. S. participation in the Korean conflict. The occupational

titles were judged concerning the degree of prestige each was accorded in American society.

A second experimental group ( $N = 94$ ) judged Set II of the attitude items (containing the seven favorable statements) as well as the set of occupational titles. This group received the same instructions as the first experimental group.

Scale values were computed for the three sets of objects judged. In addition, a coefficient, Zeta, was computed for each set of judgments obtained.<sup>2</sup> This required the computation of two coefficients for each judge indicating his consistency in comparing (a) the occupational titles, and (b) one of the two sets of attitude items.

#### RESULTS

The scale values of the nine items included in Set I of the attitude statements ranged

TABLE 1. FREQUENCIES OF COEFFICIENTS OF CONSISTENCE FOR THE JUDGES IN TWO PANELS JUDGING ATTITUDE ITEMS

Coefficients of Consistence	Panel I, Judging Nine Items, Both	Panel II, Judging Seven Favorable Items	Totals.
	Favorable and Unfavorable		
1.000	42	43	85
.850 to .999	30	27	57
Less than .850	6	24	30
Totals	78	94	172

from 0.00 to 2.66. The range of values for the seven favorable items in Set II was 0.00 to 1.71. The average scale distance between all possible pairs of values in the two sets of items was 1.12 and 0.63, respectively. The distributions of the coefficients of consistence for both sets of judgments were found to be highly skewed. Therefore, comparisons between the experimental panels was made by using chi square rather than some parametric statistic.

The comparison of the consistency of the two panels was based on the data shown in Table I. The chi square for this distribution is 9.57, which for two degrees of freedom is significant beyond the .01 level of confidence.

It might be argued that this difference may be due to a difference in the discriminability

<sup>1</sup>L. L. Thurstone, "A Law of Comparative Judgment," *Psychological Review*, 34 (1927), pp. 273-86.

<sup>2</sup>M. G. Kendall, *Rank Correlation Methods*, Charles Griffin & Co., Ltd., London, 1948, pp. 121-125.



of the two panels, rather than to the finding that the items in Set I were psychologically further apart than the items in Set II. Data bearing on the relative applicability of these two interpretations were available. The coefficients of consistence computed for the judgments of the nine occupations were compared for the two panels. The resulting chi square was 0.11, which would be expected to occur by chance approximately 95 times in 100. This finding indicates that difference in the discriminability of the two panels does not account for the previously noted difference of consistency.

In order to gain additional information on the relationship of the psychological difference between objects and the consistent judgment of those objects, the occurrence of inconsistencies in the judgments of the first set of attitude items was further analyzed. For each item triad, the scale distance between the extreme members of the triad was computed. Then, the frequency of occurrence of circularity for each of the triads was observed. It was hypothesized that there would be a negative relation between frequency of occurrence of circularity and the distance between the extreme members of an item triad. Table 2 summarizes the data utilized in this analysis.

TABLE 2. FREQUENCY OF OCCURRENCE OF CIRCULARITY AND THE SCALE DISTANCE BETWEEN EXTREME MEMBERS OF A TRIAD

Frequency of Occurrence	Scale Distance Between Extreme Members of the Triad			Totals
	0.31- 1.30	1.31- 1.90	1.91- 2.70	
2 or more	15	4	0	19
1	4	13	9	26
0	6	9	24	39
Totals	25	26	33	84

The chi square for the distribution in Table 2 is 36.93 which for four degrees of freedom is significant beyond the .001 level of confidence. The contingency coefficient, using Cramer's correction, is .47. The results of this analysis corroborate the previous finding that the greater the psychological distance between objects, the less likely these objects are to be judged inconsistently.

It was also hypothesized that an individual who was inconsistent in his judgments of one set of objects would tend to be inconsistent in his judgments of a second set of objects. It was found above that inconsistency of judgment was in part dependent on the psychological distance between the objects judged. Therefore, in order

to study individual differences in discriminability some control should be exercised over the psychological separation of the objects judged. The average scale distance between all possible pairs of the occupational titles was found to be 0.61. This difference is comparable to the value for Set II of the attitude items, which was 0.63. Therefore, it was assumed that equal discriminability was required in making consistent comparisons for each of these sets of objects.

For the 94 judges in the second experimental group, the coefficients of consistence for the judgments of the occupations were compared with those for the attitude items. Table 3

TABLE 3. COEFFICIENTS OF CONSISTENCE FOR THE JUDGES IN ONE PANEL JUDGING TWO SETS OF OBJECTS

Coefficients of Con- sistence for the Judg- ments of Occupations	Coefficients of Consistence for the Judgments of Attitude Items			Totals
	1.000	.857 to .927	Less Than .857	
1.000	21	12	4	37
.933 to .967	16	9	7	32
Less than .933	6	6	13	25
Totals	43	27	24	94

shows the data upon which this comparison was based. The chi square for this distribution is 13.996, which for four degrees of freedom is significant beyond the .01 level of confidence. The contingency coefficient is .27, after Cramer's correction has been made. Thus, there is some indication that an individual who judges one set of objects inconsistently tends to judge a second set of objects inconsistently.

#### THE CONSISTENCY OF SUBJECT BEHAVIOR AND THE RELIABILITY OF SCORING IN INTERACTION PROCESS ANALYSIS \*

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Work carried out by Bales and associates at Harvard has indicated that the *between observer*

\* This research was supported in part by the United States Air Force under contract number AF 33(038)-12782 monitored by the Human Re-

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reliability (for highly skilled observers) by session by scoring category for initiated behavior is acceptably high, with correlations ranging between .75 and .95, depending on the scoring category.<sup>1</sup> It may reasonably be inferred that for comparable conditions self-self observer reliability will be equally high or higher. For the self-self observer reliability to be lower, it would require, essentially, that the non-systematic errors of two observers consistently be in the same direction, which is relatively unlikely.

TABLE 1

Category	Test-Retest Pearson r.
1	.98
2	.93
3	.96
4	.81
5	.92
6	.96
7	.89
8	.65
9	.83
10	.70
11	.92
12	.88
Total	.92

Adequate tests of self-self observer reliabilities will have to wait until series of standardized materials (such as films) are produced for scoring and then rescored (test-retest). Tests of self-self observer reliability are restricted at present to recordings and written protocols. Experience in both these as reported by Bales and associates is satisfactory, but actual formal tests have been extremely limited since the demands on the observers have been sufficiently great that repeat scoring is ordinarily defined as a luxury item. Repeat scoring by trainees has usually demonstrated satisfactory reliabilities after three to four months of relatively intensive training. In a recent research project with the Air Force, one observer (Borgatta) required estimates of self-self reliability with written protocols. The protocols were responses to

the *Conversation Study*, a projective-type form in which ten pictures (depicting three-man groups) are presented and the subject is asked to write the conversation which is going on. The ordinary writing time is two hours. The *Conversation Studies* of eight subjects were scored and then rescored after an interval of more than four weeks. The scorer had had about four months of training at the time. The Pearsonian correlations are indicated in Table 1. Results of this order, while fairly satisfactory, are not considered to represent the best practically attainable. With greater experience specific improvement would be expected in terms of the establishment of more arbitrary criteria for making decisions, especially in terms of the categories in which known confusion of placement occurs.

It would appear, thus, that researchers may plan to utilize formal scoring techniques such as Bales' Interaction Process Analysis with a reasonable confidence that training will produce observers who are reliable (or consistently arbitrary) in their scoring. A second question of reliability has not been considered in relation to Interaction Process Analysis, and this is that of the reliability of the "test" or the consistency of the observed phenomena. In raising this question it should be noted immediately that researchers working with the observation of groups would be greatly disturbed if they found extremely high reliability of the "test" or consistency of the observed phenomena under conditions which they suppose must vary. This is especially obvious in the analysis of phase changes within a given session, session to session changes, and more generally, in the expectations (or hypotheses) concerning the development of structure in the group over time. On the other hand, if common elements exist in the conditions under which the behavior occurs (*i.e.*, the task, subjects, size of groups, *etc.*), a certain degree of consistency in the interaction pattern may be expected. It is apparent that in this type of study the term "reliability of the test" becomes inapplicable and the more correct identification is the "consistency of the observed phenomena." Few data have been produced in this area, although data which may be so analyzed in the future are currently being collected by Bales. Interest in the consistency of performance of subjects led to a limited treatment of some data in Bales' files. Five sets of data, each consisting of two or four sessions in which the same persons participated were analyzed to ascertain the stability with which each person maintained his position in relation to the other members of the session. In the case of four sessions, sessions 1 and 3, and 2 and 4, were

sources Research Institute, Maxwell Air Force Base, Alabama. Permission is granted for reproduction, translation, publication and disposal in whole or in part by or for the United States Government.

<sup>1</sup> Heinicke, Christoph, and Bales, Robert F. "Developmental trends in the structure of small groups," *Sociometry*, XVI (February, 1953), pp. 7-38.

TABLE 2

Number of participants (N)	3	5	5	4	5
Number of sessions	2	2	4	4	4
Category 1	.40	-.14	.58	.26	.13
2	-.62	.67	.94	-.15	.50
3	.93	.91	.70	.95	.73
4	.26	.79	.83	.84	-.13
5	.87	.84	.94	.91	.58
6	.80	.79	.98	-.01	.76
7	.97	.39	.86	.01	.33
8	.92	.27	.99	.96	.19
9	-.03	.01	.33	.97	.61
10	-.53	.68	.69	.97	.74
11	.02	.85	.78	.89	.23
12	.00	-.25	.96	.00	1.00
Total	.99	.96	.85	.92	.56

combined directly. (A minor error is introduced in that the sessions were not exactly of the same length. This could have been corrected by conversion to time rates, but the additional work did not seem warranted by the additional accuracy which would have been evident.) The Pearsonian correlations are indicated in Table 2. The variance expected in correlations with such small N's is large, and in addition to this the number of scores in certain categories is quite small as in the case with category 2. At the same time, the general picture of the correlations indicates that a positive relationship exists between the behavior of the same subject from one time to another in all the categories. This is not a sufficient test of the stability of individual performance, but is sufficiently good to give the researcher some confidence. The correlations

may be viewed as depressed by certain known factors (session to session changes, development of a status hierarchy, task changes, and many additional "interference" factors).

Again, in connection with a research project for the Air Force, a sample of 126 subjects was observed, using Bales' scoring system. The sample was subdivided into 14 batches of nine subjects each. Within a batch of nine, each subject participated in groups of three four times, each session consisting of 48 minutes of observation time. Each subject participated with each of the eight other subjects of the batch of nine, two at a time. Each session was divided into six units of time and two general types of activity as follows:

Actual behavior—get acquainted—six minutes  
 Actual behavior—plan role playing—six minutes  
 Role playing behavior—role playing—twelve minutes  
 Actual behavior—plan role playing—six minutes  
 Role playing behavior—role playing—twelve minutes  
 Actual behavior—relax—six minutes

Data were collected by time unit, permitting collation under several plans. The "actual behavior" of each subject consisted of four units of six minutes each for each of four sessions (a total of 96 minutes). Each session involved the same type of task, but presented a different social situation for the subject since in each session he participated with two new persons for the first time. The 126 subjects may thus be examined under similar conditions for the stability (or consistency) of their social behavior (as scored by the Bales system) which may be attributable to individual personality factors. The stability of subjects was examined by two plans: (1) The patterns of behavior of the subjects in sessions one and three, and two and

TABLE 3. STABILITY OF SUBJECTS BY CATEGORY BY TYPE OF BEHAVIOR OBSERVED (126 SUBJECTS)

Category	Initiated Role-Playing		Initiated Actual		Received Role-Playing		Received Actual	
	Plan (2)	Plan (1)	Plan (2)	Plan (1)	Plan (2)	Plan (1)	Plan (2)	Plan (1)
1	.38	.33	.30	.23	.40	.23	.28	.26
2	.60	.15	.79	.43	.30	.25	.71	.14
3	.79	.64	.76	.63	.64	.22	.70	.41
4	.50	.46	.71	.61	.43	.31	.59	.34
5	.67	.47	.80	.59	.56	.29	.68	.49
6	.66	.65	.72	.57	.41	.28	.53	.29
7	.49	.28	.53	.53	.22	.13	.18	.07
8	.48	.46	.37	.34	.21	.14	.16	.09
9	.43	.13	.21	.00	.26	.08	.09	.10
10	.47	.44	.51	.32	.51	.31	.54	.27
11	.56	.34	.72	.43	.56	.29	.54	.36
12	.62	.11	.61	.02	.35	.13	.56	.10
Total	.79	.63	.81	.57	.57	.33	.78	.50



four, were combined and correlated. In this case we compare the behavior of the same individual in two situations with two other situations, masking changes that may be present within sessions<sup>2</sup> and revealing stability between sessions that may be present (in the behavior of the individual) in spite of the fact that different persons are present in the group. (2) In the second plan we compare the behavior of the same subject in one half of each of the four sessions with his behavior in the other half, thus masking the changes between sessions and revealing the stability over time within sessions when the same two other persons are present. The correlations for initiated actual, initiated role playing, received actual, and received role playing behavior are indicated in Table 3.

The plan (1) correlations are ordinarily lower than the plan (2) correlations. This may be interpreted as evidence for the hypothesis that a person will be more consistent, given the same amount of time, when interacting with the same individuals than when interacting with different individuals. The stability of subjects shown is sufficient to encourage us to believe that the interaction of an individual, as scored by this system, may tell us something about his personality, in spite of peculiarities due to the fact that he is interacting with particular other persons.

<sup>2</sup> Bales, Robert F. *Interaction Process Analysis*, Addison-Wesley Press, Cambridge, 1950.

#### THE DISTRIBUTION OF FREE-FORMING SMALL GROUP SIZE \*

JOHN JAMES

*University of Oregon*

This paper is a report of further analysis of small group data secured in a 1950 field project.<sup>1</sup> The consistent regularities displayed by the series of distributions of free-forming small group size suggested the possibility that the empirical distributions could be fitted by a statistical model and thereby converted to more general form.

\* The writer is indebted to Professor W. J. Dixon for statistical counsel and for a critical reading of the paper, and to Miss Susan F. Huffaker for field and laboratory assistance. A grant by the Graduate School, University of Oregon, made the study possible.

<sup>1</sup> See John James, "A Preliminary Study of the Size Determinant in Small Human Group Interaction," *American Sociological Review*, 16 (August, 1951), pp. 474-77.

Free-forming small groups are those whose members are relatively free to maintain or break off contact with one another, that is, they are ones where informal controls on behavior are at work and spontaneity is at a maximum. The interaction between the members is face-to-face and is manifested overtly by gesture, laughter, smiles, talk, play, or work.

Field observations<sup>2</sup> were conducted in Eugene and Portland, Oregon in the winter and spring, 1950 in the course of which 15,486 observations of free-forming small groups were made and arranged by type of group, place and time in 18 empirical distributions. These 18 frequency distributions were consolidated into one and gave the following J-shape summary:

Group Size	Frequency	Per cent of total
1	10,149	65.54
2	3,945	25.47
3	1,075	6.94
4	238	1.54
5	65	.42
6	14	.09
N	15,486	100.00
Mean	1.46	

All 18 component distributions of the above, except one (public beach picnic area), were J-shaped. The data for size 1 were included in the distributions in order to account for every individual in the field of action under observation.

Two models, the negative binomial and the Poisson, were fitted to each of the 18 frequency distributions and the goodness of fit tested by chi-square, the results of which are given in Table 1.

According to Table 1, the hypothesis, that the observed frequencies correspond to the theoretical frequencies, is acceptable for the two models is summarized in Table 2.

The summary shows that the test of correspondence for the negative binomial as determined by chi-square was acceptable in 94.4 per cent of the distributions (17 of the 18 distributions) at the .05 level of significance and in 100 per cent of the distributions at the .01 level. We may, therefore, at least tentatively regard

<sup>2</sup> All observations were carried out by teams of two, namely, an observer and a recorder. The period of observation for most types of group activity was one or more hours, the greatest number being seven hours for Portland pedestrians. Observations for a few types of group activity had to be limited to one-half hour each.

TABLE 2. PER CENT OF FITTINGS ACCEPTABLE FOR FREE-FORMING GROUP SIZE DISTRIBUTIONS

Model	Level of Significance	
	.05	.01
Negative Binomial	94.4	100.0
Poisson	61.1	77.8

the negative binomial as an appropriate model for empirical distributions of free-forming small group size. The outcome of the test for the Poisson, while suggestive, represents too wide a discrepancy between the observed and theoretical frequencies to consider its acceptance in the present circumstance.

A further word may be said about the differential chi-square results of the negative binomial and Poisson fittings. The Poisson model

requires a constant mean throughout the observing period. It would be expected, therefore, that this model would fit those distributions deriving from social situations where the relationships governing the combinations of individuals were relatively stable. The negative binomial, in contrast, can be considered as representing a family of different Poisson distributions collected together and thus appears to be the more suitable model for diverse empirical distributions of the kind represented by our data. It would seem, therefore, that the Poisson would fit more often if observations were restricted to classes of groups that were relatively homogeneous by virtue of deriving from functionally similar situations where the individual's freedom to interact or not was near the maximum. Additional data for the further statistical investigation of small group size ought preferably to be obtained under experimentally controlled conditions.

TABLE 1. CHI SQUARE TEST FOR GOODNESS OF FIT OF NEGATIVE BINOMIAL AND POISSON MODELS TO FREQUENCY DISTRIBUTIONS OF FREE-FORMING SMALL GROUPS

Type of Group, Place, Time	Number of Groups Observed	Group Size Range	Chi square for Negative Binomial	Chi square for Poisson
1. Pedestrians—Eugene, Spring, Morning	2,423	1-6	0.88	12.20**
2. Pedestrians—Eugene, Spring, Afternoon	1,840	1-6	3.36	7.35*
3. Pedestrians—Eugene, Winter, Morning	591	1-6	1.35	5.12
4. Pedestrians—Portland, Spring, Morning	2,080	1-5	0.25	35.21**
5. Pedestrians—Portland, Spring, Afternoon	1,501	1-5	3.97*	0.49
6. Shopping Groups—Eugene, Spring, Dep't Store and Public Market	510	1-5	0.45	2.70
7. Shopping Groups—Eugene, Winter, Dep't Store	206	1-6	0.20	5.63
8. Shopping Groups—Portland, Spring, Two Dep't Stores	1,356	1-5	2.80	3.66
9. Play Groups—Eugene, Spring, Public Playground A	497	1-5	1.29	6.61*
10. Play Groups—Eugene, Spring, Public Playground B	258	1-5	0.27	1.01
11. Play Groups—Eugene, Spring, Public Playground C	455	1-6	5.11	29.21**
12. Play Groups—Eugene, Spring, Public Playground D	507	1-6	3.50	2.99
13. Play Groups—Eugene, Spring, Playgrounds of 14 Elementary Schools <sup>1</sup>	1,277	1-6	2.82	2.90
14. Play Groups—Eugene, Spring, Nursery School Ages 2-5	503	1-6	0.67	7.32**
15. Play Groups—Eugene, Spring, Nursery School Ages 4-5	133	1-5	0.34	2.76
16. Public Gatherings—Portland, Spring, Public Beach Swimming Pool	581	1-5	2.09	6.46*
17. Public Gatherings—Portland, Spring, Public Beach Picnic Area <sup>2</sup>	359	1-6	4.24	5.77
18. Public Gatherings—Portland, Spring, Railroad Depot	409	1-5	3.03	3.79
Total	15,486			

\* Significant at .05 level.

\*\* Significant at .01 level.

<sup>1</sup> Directed or organized play not included in observations.

<sup>2</sup> Fewer cases in size 1 than in size 2.

# OFFICIAL REPORTS AND PROCEEDINGS



## NOTICE CONCERNING THE 1954 ANNUAL MEETING

The meeting will be held at the University of Illinois, Urbana, Illinois, September 8, 9, 10, 1954.

In line with President Znaniecki's plans for the program, sessions are being projected on the following topics with the following chairmen:

- Sociological Theory, Pitirim Sorokin, Harvard University
- History of American and European Sociology Since World War I, Howard Becker, University of Wisconsin
- Relationships Between Sociology, History and Anthropology, Julian Steward, University of Illinois
- Interpersonal Relations, James Woodard, Temple University
- Professions, Talcott Parsons, Harvard University
- Social Groups, George Homans, Harvard University
- Family, Clifford Kirkpatrick, Indiana University
- Social Psychology, Leonard S. Cottrell, Jr., Russell Sage Foundation
- Sociology of Education, Wilbur Brookover, Michigan State College
- Population, Kingsley Davis, Columbia University
- Communities—Urban and Suburban, Richard Dewey, University of Illinois

- Communities—Small Town and Rural, Samuel Blizzard, Pennsylvania State College
- Regional Sociology, Howard Odum, University of North Carolina
- Communication and Public Opinion, J. W. Albigh, University of Illinois
- Industrial Sociology, E. William Noland, University of North Carolina
- Social Stratification, Charles Page, Smith College
- Sociology of Law and Political Organization, Philip Selznick, University of California, Berkeley
- Social Disorganization, Robert E. L. Faris, University of Washington
- Criminology, George B. Vold, University of Minnesota
- Cultural and Racial Minorities, Maurice R. Davie, Yale University
- International Relations, Robert C. Angell, University of Michigan
- Sociology of Religion, William Goode, Columbia University
- Sociology of Knowledge, Robert K. Merton, Columbia University
- Sociology of Literature, Art and Music, John Mueller, Indiana University
- Methodology, Theodore Abel, Hunter College

Although further details on the program will be sent to the membership, any member who wishes to propose a contribution to the program is urged to communicate directly with the chairman of the appropriate session at an early date.



## COMMUNICATIONS AND OPINION



### *To the Editor:*

Having recently completed some research on "Interracial Marriages in Washington, D. C., 1940-47" in an unpublished Ph.D. dissertation at Catholic University and having made an earlier study in 1949 for a Master's thesis on "Fifteen Negro-White Marriages in New York City and the Metropolitan Area", this writer found the article by Joseph Golden on "The Negro-White Intermarried in Philadelphia" in the April 1953 issue of the *Review* to be of special interest. There are some observations, however, which she would like to make.

There was, in particular, a question in my mind about an interpretation made by Dr. Golden, viz. re "In the case of one-third of the whites involved in these interracial marriages, we may say that one of the factors which facilitated intermarriage was the lack of knowledge of the American disapproval of such intermarriages".

In my own contacts with white war brides married to Negro G.I.'s the brides have in each instance stated that they were very much aware of American disapproval of Negro-white intermarriages. Most of the wives seemed quite relieved that the situation in New York City is not nearly so bad as they had expected it to be. One war bride said she first heard about the status of the Negro in the United States from some white G.I.'s who were in Belgium before any Negroes arrived. She received the impression that even in New York Negroes and whites are segregated on the subways, and that on the buses the Negroes had to sit on the upper deck while the lower deck was reserved for whites. One G.I. informed her that if a white woman married a Negro, then a Negro woman would probably kill her. When she decided to marry her husband despite what she had heard, one officer in her husband's outfit tried to dissuade her. This officer was from New York and the wife said she feels certain that he presented the situation as he saw it but she, having been in this country for three years at the time of the interview, felt that the officer exaggerated the situation. He told her she would lose everything by marrying a Negro and suggested that she marry a white G.I. Other foreign-born white women from Germany and Austria as well as additional war brides from Belgium and England stated that

they had heard about American disapproval of interracial marriages before they were married but only one felt that the situation was worse than she expected it to be. In this instance the Negro husband was a successful business man with a white business partner. On several occasions white employees had threatened to quit work if the white partner did not fire her Negro husband. In these instances the white partner told the employees they could leave any time but that he was remaining in business with the Negro partner. The wife felt that this was because her husband is honest and a good business man. The couple lived on Riverside Drive and when the wife attempted to take the children to amusement parks she was on occasion insulted when questioned about her children. She said she did not know such "intolerable ignorance" could possibly exist. Even the latter, however, asserted that he knew about American disapproval of interracial marriages.

It seems to this writer that when referring to the foreign-born white women who intermarried with the Negro, one of the factors which facilitated intermarriage was in the area of emotions rather than a lack of knowledge of the American disapproval of interracial marriages. That is, the foreign-born white women are free from an adverse emotional reaction to intermarriage with the Negro. This interpretation is drawn only from my own limited contacts with foreign-born whites who have intermarried with the Negro, all of whom are women. It might be noted that two of the foreign-born whites married to Negroes who were married in Washington, D. C. between 1940-47, have established residence in Philadelphia and both of these were likewise aware of American disapproval of interracial marriages at the time of their marriage.

SISTER ANNELLA LYNN

*Catholic University*

### *To the Editor:*

Most members of our tribe are obliged to reconcile at least two professional roles. We are educators: and we hope to continue as practicing sociologists. We are intimately bound to the requirements of university life. Yet we have, presumably, strong allegiance to the Society's

objectives. While the boundaries of the two roles are not incompatible, they are by no means congruent. On the student side, the conventional classroom activities are scarcely sufficient to produce a keen concern with the process of social inquiry as demonstrated in the current work of sociologists.

During the next academic year I shall ask senior students in my social theory class to subscribe to the *American Sociological Review*, using it as a major source. While this procedure has obvious limitations (long range planning and systematic organization will be difficult, the materials may be forbidding for undergraduates) there should also be distinct advantages. We will be making use of the best available agency for sifting and presenting current professional work. We will be confronted with a natural rather than a contrived learning situation. The problem will be to set research findings in a systematic theoretical framework which will maximize their relevance, meaning and implications. Such a procedure should help us establish standards demanding honest application, critical thinking and the development of certain minimum skills. It should suggest broad research areas within which a prospective graduate student may begin to develop particular interests. Through student affiliation in the society there should emerge a sense of identification with those who are facing the most difficult of all research—gaining reliable knowledge about human conduct. Obviously, this kind of operation must be a challenge to the instructor.

Such a course would clearly make unfamiliar demands upon the instructor. In view of the variety of materials dealt with, the class must hold fast to an unambiguous definition of the central task—theoretical formulations embracing differing systems of social relationship. Some kind of framework, taxonomy or codification will be necessary to unify the disparate readings. A core of central concepts should be at hand, to be mobilized as they become relevant. The instructor must have a preparatory posture allowing the introduction of pertinent materials both from major theorists and from other relevant research. He would undoubtedly do well to draw upon his colleagues for special contributions along the line. In short, he should be prepared for some fancy pedagogical footwork. Perhaps above all he should have sufficient humility to temper his objectives and anticipations for the course: and to make it a genuine collaborative enterprise starting, as he must, with the same fresh materials as do his students.

I would appreciate criticism from colleagues and suggestions for evaluating this effort.

EVERETT K. WILSON

*Antioch College*

*To the Editor:*

I hereby protest the "Editor's Note" on page 771 of the December 1952 issue of the *American Sociological Review* and the decision of the Editor and the Executive Committee of the Society "to depart from custom and publish a second review" of *Understanding Public Opinion* by Professor Curtis D. MacDougall.

Despite the limit of 400 words set by the Book Review Editor, my original review of the book on pages 642 and 643 of the October 1952 issue of the *American Sociological Review* followed the "principles" enunciated by the Executive Committee:

1) I agree that this is a "period of hysteria"; the action of the Executive Committee is an example of it. I did not "irresponsibly" threaten Professor MacDougall's "human rights." I assume it is not part of a "social scientist's" human rights that book reviewers must review his books favorably. On this score, the main difference between my review and the second review is that the second includes some qualified words of praise although in the first paragraph the second reviewer says students of journalism would be better off with a different kind of book and in the last paragraph that the book is not suitable for social science classrooms. In other words, it is all right with the Executive Committee to write an unfavorable review of a book if you sprinkle in a few polite phrases. Does freedom of speech for the American Sociological Society amount to freedom of double talk?

The only possible objection to my review having anything remotely to do with human rights was that I cited some of the Communist fronts with which Professor MacDougall has been affiliated. These are all a matter of public record, many appearing in official publications of the United States government. Professor MacDougall has not repudiated them; on the contrary, I believe he is proud of them. Professor MacDougall's Communist-front record is relevant to an evaluation of the book and to the next point.

2) The second reviewer of the book agrees with me that the book is biased: "The author's predilection for the extreme statement, the positive, unqualified assertion of definite opinion, his inability to see about him the many positive forces counteracting the negative aspects of American culture, the concentration upon the

exploitive rather than the altruistic, all indicate the author's own cynical and polemic approach." (*American Sociological Review*, April 1953, page 218).

I suppose it is more polite to refer to the author's "polemic approach" than, as I did, to his "radical propaganda." *Webster's Unabridged Dictionary* defines *polemic* as "of the nature of, pertaining to, or involving an aggressive attack on, or the refutation of, others' opinions, doctrines, or the like; controversial." A "polemic" book must be biased.

The Executive Committee stated that "direct evidence of how alleged bias affects the content of a book should be cited and documented if the charge of bias is made." Even within the confines of my 400-word review, I offered some documentation. The second reviewer, given more than three times as much space, naturally was able to do this more completely. If the Editor would like a third review, more damaging examples can be given.

The pronouncement of the Executive Committee apparently applies retroactively only to me. Under the same Editor and two-thirds of the same Executive Committee, a reviewer, Professor Oliver C. Cox, in the issue of August 1952, in a review of *Communism Versus the Negro* charged bias against the author, Father William A. Nolan, in the following words:

Today, the communists of the United States are being attacked on many fronts; and, partly because the salient of this movement has become

official policy, there seems to be a certain prestige attached to one's private efforts. "This book," according to its author, "aims to convict the Communist party through the medium of its own dialectical deceptions." Perhaps it may be of some pertinence to note that the writer is a member of a research staff of the Institute of Social Order in Saint Louis University, a Jesuit school.

As it stands in the context of the disapproval of my review, the statement of the Executive Committee serves notice to future reviewers not to refer to authors' connections with the Communist movement. In effect, the Executive Committee warns reviewers not to label Communist propaganda as such. I submit that this is a limitation of freedom of speech which has no justification whatsoever. On the contrary, the Executive Committee should encourage reviewers to identify propaganda masquerading in the guise of objectivity and to engage in the freest possible kind of discussion of controversial ideas and behavior.

To this end, the Executive Committee might well revise its principles, enunciated in the December 1952 *Review*. As a beginning, I would like to suggest the principle or policy that books on Communism not be assigned for review to persons with Communist-front records, unless so identified by the Editor in the *Review*.

I request the publication of this letter in an early issue of the *American Sociological Review*.

RUTH A. INGLIS

New York City

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## NEWS AND ANNOUNCEMENTS



**The Eastern Sociological Society** held its twenty-third annual meeting on March 28th and 29th, 1953, at Harvard University. The presidential address, "The Place of the Individual in a Highly Organized Society," was delivered by President Wilbert E. Moore at the annual dinner. Professor Walter Metzger of the Department of History, Columbia University, gave the guest address on "Academic Freedom in Historical Perspective."

Session meetings consisted of two sections on Current Research Reports chaired by Edward C. Devereux, Jr., and William J. Goode; one on Research in Mental Health chaired by A. B. Hollingshead; one on Community Studies of Some Effects of Rapid Industrialization chaired by Henry J. Meyer; one on Research on Soviet Social Structure chaired by Alex Inkeles, and one on Studies in Social Organization chaired by Ira De A. Reid. Leonard S. Cottrell, Jr., served as moderator for a panel discussion of Russell Sage Foundation experience entitled Bridging the Gap Between Social Science and Social Practice. A tour-demonstration of the Laboratory of Social Relations was arranged by Samuel A. Stouffer.

Newly elected by mail ballot were Ira De A. Reid, Haverford College, as President; Robin M. Williams, Cornell University, as Vice President; Alfred M. Lee, Brooklyn College, as Representative to the Council of the American Sociological Society; and James Barnett, the University of Connecticut, as a member of the Executive Committee for a three year term.

The Department of Social Relations at Harvard was host for the meeting. Joseph Kahl of the Department served as chairman of the Committee on Local Arrangements.

**The Ohio Valley Sociological Society** officers elected for 1953-54 are: Harold T. Christensen, Purdue University, President; Leonard C. Kercher, Western Michigan College, Vice President; Harold A. Gibbard, West Virginia University, Secretary-Treasurer; and C. T. Jonassen, Ohio State University, Editor.

**The Pacific Sociological Society** held separate meetings of the Northern, Central and Southern Divisions during the month of April. The Southern Division met at Whittier College on April 4 with approximately 75 persons in attendance. Research papers were presented and discussed and Charles B. Spaulding from Santa Barbara College, Vice-President, presented a luncheon address. The Central Division met at the College of the Pacific on April 10 with approximately 75 persons in attendance. In charge of the meeting was Robert A. Nisbet, formerly from the Berkeley campus and

now from the Riverside campus of the University of California. A luncheon address was given by Herbert Blumer, Chairman of the Department of Sociology at the University of California, Berkeley. The Northern Division met at Gearhart, Oregon on April 23 and 24.

In addition to its divisional meetings the Pacific Sociological Society is cooperating with the American Sociological Society in connection with the annual meeting at Berkeley, August 30 through September 2.

**The Southern Sociological Society** held its Sixteenth Annual Meeting at Chattanooga, Tennessee, March 26-28, 1953. Officers elected for the ensuing year were: President, Guy B. Johnson, University of North Carolina; President-elect, Morton B. King, University of Mississippi; First Vice-President, Irwin T. Sanders, University of Kentucky; Second Vice-President, Dorothy Jones, Winthrop College; Secretary-Treasurer, Melvin J. Williams, Stetson University. Haskell M. Miller, University of Chattanooga, and Solon T. Kimball, University of Alabama, were elected to serve three-year terms on the Executive Committee.

Continuing to serve as elected members of the Executive Committee are: Abbott L. Ferriss, Human Resources Research Institute; Preston Valien, Fisk University; Selz C. Mayo, North Carolina State College; and Vernon J. Parenton, Louisiana State University.

Serving on the Executive Committee as past presidents are Wayland J. Hayes, Vanderbilt University; Lee M. Brooks, University of North Carolina; H. C. Brearley, George Peabody College; Rudolf Heberle, Louisiana State College; and Leland B. Tate, Virginia Polytechnic Institute.

Katherine Jocher, University of North Carolina, continues to serve as representative to the American Sociological Society Executive Committee.

**National Science Foundation.** Five new members have been appointed to the staff of the National Science Foundation by Dr. Alan T. Waterman, director of the Foundation. Among those is Harry Alpert, study director for social science research, Program Analysis Office, from the Bureau of the Budget.

**Columbia University** announces a program of graduate studies in social psychology leading to the Ph.D. degree. Members of the departments of Psychology, Sociology, Education and Anthropology are cooperating in offering this interdisciplinary curriculum. The administrative committee consists of professors Otto Klineberg and Goodwin B. Watson, co-chairmen; Conrad M. Arensberg, Hubert

Bonner, Kenneth F. Herrold, Herbert H. Hyman, Paul F. Lazarsfeld, Irving Lorge and S. Stansfeld Sargent. A limited number of candidates for the degree (not more than ten or twelve) will be selected on the basis of high intelligence, excellent scholarship, strong interest, and promise of achievement in a career in social psychology. Qualifying entrance examinations will be given later in the summer on dates to be announced. For more complete information write to the Director of Admissions, Columbia University, or to Prof. S. S. Sargent, Barnard College, Columbia University, New York 27, New York.

**Brooklyn College of the City of New York.** As of January 1, 1953, Willoughby C. Waterman was promoted to full professorship and Samuel Koenig to associate professorship. Rex D. Hopper was promoted to associate professorship on November 1, 1952. Marion Cuthbert is to become an associate professor on January 1, 1954.

Associate Professor Herbert H. Stroup will serve as Director for Greece, Congregational Christian Service Committee, during his 1953-54 sabbatical leave. He will be located at Pierce College in Athens.

Assistant Professor LeRoy Bowman receives his Ph.D. from Columbia University in June, 1953.

Dr. Jerome Himelhoch has been appointed Editor of *Social Problems*, official quarterly journal of the Society for the Study of Social Problems, by Professor Arnold M. Rose, Chairman of the Editorial Committee. Rose has also appointed Sidney Aronson as Business Manager and Dr. Nathan Gerrard as Advertising Manager. Professor Samuel Koenig represents the Editorial Committee on the board of *Social Problems*.

Associate Professor Feliks Gros served as Visiting Professor in the University of Virginia during the Spring 1953 semester. He is also Adjunct Professor at New York University.

Dr. Charles R. Lawrence, Jr., will offer two seminars in the 1953 graduate summer school of the four city colleges at Hunter College. They will be in "Culture and Personality" and "Minority Groups in the United States."

**Brown University.** The Department of Sociology has inaugurated a doctoral program in the areas of population, ecology, and regional development. Fellowships for doctoral candidates in these areas will be available each year. R. Glenn Kume-kawa, formerly a graduate assistant in the Department, has been appointed a University Fellow for 1953-54.

Charles M. Grigg, assistant professor, was in charge of the Statistics Laboratory at the University of North Carolina during the summer term.

Kurt B. Mayer has been promoted to associate professor and Vincent H. Whitney to full professor.

**University of California at Berkeley.** Professor Reinhard Bendix will give a paper on "The Legitimation of an Entrepreneurial Class," at the World Congress of Sociology to be held at Liège, Belgium, in August 1953.

Professor Kenneth E. Bock has received a Faculty Fellowship under the Fund for the Advancement of Education and will spend the academic year, 1953-54, observing and studying teaching procedures in small classes and other means of establishing close faculty-student relations in instruction and training situations.

Charles Woodhouse, Acting Instructor for the 1952-53 academic year, has accepted an appointment as Instructor in Sociology on the Riverside Campus of the University of California, beginning January 1954. He has previously been Instructor in Sociology at Idaho State College from 1950-1952.

**University of Chicago.** Robert J. Havighurst will be Fulbright Professor of Education at the University of New Zealand for a year commencing July 1, 1953. He will lecture on "General Education in American Colleges." He will also conduct a research seminar on "Social Structure and Education" and he hopes to start some community studies which will give comparable data to the studies of social structure and education which have been made in the United States.

William E. Henry, Associate Professor of Psychology and Human Development, is the new Chairman of the Committee on Human Development of the University of Chicago.

**Bates College.** Peter P. Jonitis has been appointed assistant professor of sociology. For the past year he was a post-doctoral guest scholar at the Russian Research Center, Harvard University. As a member of the Project on the Social System of the Soviet Union, his research was concerned with the demographic characteristics and the ideological content of political organizations of recent Soviet émigrés.

**University of North Carolina.** Howard W. Odum was the recipient in March of the O. Max Gardner Award which goes to "the member of the faculty of the Consolidated University of North Carolina who in the current scholastic year has made the greatest contribution to the welfare of the human race." In making this fifth annual award—as provided in the will of the late Governor who brought about the consolidation of the three units at Chapel Hill, Greensboro, and Raleigh—the Board of Trustees honored him as teacher, founder, and builder, as versatile writer and scholarly researcher, whose works in literature and in the sociology of regionalism and folk culture have helped "mightily to awaken and rebuild his beloved South and thus to strengthen the Nation."

Guy B. Johnson was elected President of the Southern Sociological Society for 1953-54 at the 16th annual meeting held in Chattanooga in March.

Professors who were away from Chapel Hill for teaching or research in the summer of 1953: Harold D. Meyer at the University of Colorado; Rupert B. Vance at Columbia University; John P. Gillin at the University of Wisconsin; Nicholas J. Demerath at Harvard University; and E. William Noland on special research at the Maxwell Air Base, Alabama.

Visiting Professors at Chapel Hill for the summer of 1953: Joseph K. Balogh from Bowling Green State University (first session); James E. Fleming from Kent State University (second session); Charles M. Grigg from Brown University (first session); Albert E. Lovejoy from Lynchburg College (second session).

Reuben Hill taught in the Workshop of Family Life Education at the University of Minnesota in the early summer of 1953. Professor Hill, on leave at the University of Puerto Rico for the year 1953-54, is directing at the Social Science Research Center the second phase of research in family dynamics and human fertility, following up the work started in 1951. He is also teaching family seminars.

Gerald R. Leslie of Purdue University is Visiting Assistant Professor replacing Professor Hill for the year 1953-54.

Gordon W. Blackwell spent part of the summer on a study of communities in disaster for the National Research Council.

Joffre L. Coe has returned from a year's study at the University of Michigan to resume the direction of the Laboratory of Archaeology and Anthropology. John E. Heimwick, who served in Mr. Coe's place as Visiting Instructor during 1952-53, returned to the University of Michigan for continued study.

Graduate students in sociology who have recently received teaching appointments: James W. Green to North Carolina State College (Raleigh); John T. Greene to Boston University; James J. Maslowski to the College of William and Mary; J. Noel Moss to the University of West Virginia; O. Norman Simpkins to Bowling Green State University; Ram Singh to Hislop College of Nagpur University, Nagpur, M.P., India to organize a Department of Sociology and Anthropology; and Kenneth C. Wagner to George Institute of Technology.

**University of North Dakota.** Peter A. Munch, Head of the Department of Sociology and Anthropology, is serving as chairman of a Social Science Research Committee which has been established at the University of North Dakota. It consists of ten members, two from each of the departments of Economics, Geography, History, Political Science, and Sociology. The members are appointed by the President of the University. The purpose of the Committee is to stimulate and sponsor research in the Social Sciences, particularly pertaining to North Dakota and the Great Plains.

**The Ohio State University.** The name of the Department of Sociology has been changed to the Department of Sociology and Anthropology, in recognition of the increasing role played by the anthropological offerings and research.

C. T. Jonassen has been appointed director and chief investigator of a project to study urban decentralization in two cities of the United States. This project is sponsored by the National Research Council and is an extension of the study conducted by him in Columbus in 1952.

Thomas Kettig is working as a Junior Staff member on a School-Community survey sponsored by

the Kellogg Foundation which is to be a five-year study throughout the nation on better ways of correlating school and community needs. For this study, other than Ohio State, research centers are University of Chicago, George Peabody College, and the University of Oregon.

**University of Pennsylvania.** William M. Kephart and Richard D. Lambert have been appointed assistant professors, Everett Lee research assistant professor, and Sidney Goldstein and Earle Reeves instructors.

Richard D. Lambert has been appointed assistant editor of *The Annals* of the American Academy of Political and Social Science.

Arthur K. Davis, visiting professor, Columbia University, has been appointed visiting professor for the academic year 1953-54. He will teach courses otherwise taught by Professor W. Rex Crawford, who will spend a year in Europe as Director of the European Seminar in American Studies, Salzburg, Austria.

Thorsten Sellin was the official United States observer at a conference of Latin American nations convened in Rio de Janeiro during last April by the United Nations to discuss penological questions. In June he was chairman of an international ad hoc committee called together by the United Nations to discuss a research program in the field of crime prevention and treatment of offenders, and to plan the international prison congress of 1956.

**Purdue University.** The Graduate Council has given official approval of a regular Ph.D. program in sociology. This comes after six years of development at the masters level and the granting of approximately 30 M.S. degrees. Present staff members are as follows: Harold T. Christensen, Chairman, Elizabeth Wilson, Dwight W. Culver, J. Roy Leevy, J. E. Losey, Louis Schneider, Allan A. Smith, Walter Hirsch, Gerald Leslie, Hanna Meissner, and Rilma Buckman. Applications for Graduate Assistantships are being received.

Gerald Leslie has been granted a one year leave of absence to substitute for Reuben Hill in family sociology at the University of North Carolina.

**The University of Rochester Press** was established a few months ago with the aid of a special grant. It will limit its publishing entirely to microtext, using the 3 x 5 inch microcard which will reproduce up to 48 pages of 8½ x 11 inch typewritten copy, and probably larger cards as they become available. Microcards sell for about twenty-five cents each, making the cost to the purchaser a little over half a cent a page. The Press is still exploring various fields to determine what it can do that will aid scholarship, either by reproducing older material that is scarce and difficult to get, or by publishing original manuscripts that are too limited in appeal to make publication in the regular way feasible because of rising printing costs.

**Tulane University.** Robert Lystad has returned to the University as Assistant Professor of Anthropology, after a year spent teaching at Northwestern.



Thomas Ktsanes, who is completing his doctoral work at Northwestern, has been appointed an instructor.

Harlan W. Gilmore is Visiting Associate Professor of Sociology at the University of Michigan for the current year.

Leonard Reissman and Warren Breed have been advanced from the rank of instructor to assistant professor.

William L. Kolb and Leonard Reissman have received grants from the Tulane University Council on Research; Kolb to design a study of the relationship between value content and levels of family integration; and Reissman to continue work on aspiration levels and vertical mobility.

**Washington University.** James B. Watson has received a Ford Fellowship grant for two years of research in New Guinea.

Jules Henry will be Visiting Associate Professor at the University of Chicago in 1953-54.

Paul Campisi has spent the year 1952-53 in Italy on a Fulbright award. He is making a comparative study of an isolated rural village and a small industrial city.

Shu-Ching Lee, now at the University of Oregon, is joining the staff in September to teach courses on the Far East.

Ralph Patrick, Jr., now at Massachusetts Institute of Technology, will join the staff in September to teach various courses including social stratification and social theory.

Nicholas Babchuck, now engaged in research in the United States Navy, under the auspices of Tufts College, will join the staff in September with special attention to human relations in industry.

**Yale University.** Theodore R. Anderson of the University of Wisconsin is joining the staff in the fall. John Sirjamaki has accepted an associate professorship at the University of Minnesota, and Fred Strodbeck will be Associate Professor of Sociology in the Law School of the University of Chicago.

Ray H. Abrams of the University of Pennsylvania has been Research Associate on the project Survey and Assessment of Areas and Methods of Research

in Nursing, under the direction of Leo W. Simmons. Carolyn Zeleny has been assisting Dr. Simmons in preparing for publication the results of his research on the application of social science to medicine at Cornell Medical Center under the sponsorship of the Russell Sage Foundation.

The following have been Visiting Research Fellows during the academic year: Dr. Palayam M. Balasundaram, Dr. Herman D. Bloch, and Dr. Helmut Schoeck.

Robert Straus, who has been Research Associate at the Yale Center of Alcohol Studies, has accepted an assistant professorship at the State University of New York, College of Medicine, at Syracuse. Charles R. Snyder is joining the staff at the Center as Research Assistant in the fall.

The following candidates for the doctoral degree have accepted appointments beginning in the fall: Russell Langworthy at Carleton College, Richard J. Coughlin at Yale University in the Southeast Asia Studies program, and Raymond Forer at the University of Connecticut.

Bernard C. Rosen, who has been research assistant in the Development of Talent Project under the direction of Professor Strodbeck, is joining the staff at the University of Connecticut in September.

Shortly before going to press, the Review received word of the death on June 1, 1953, of Erle Fiske Young, former Professor of Sociology at the University of Southern California and an Active member of the Society for many years. Dr. Young received his Ph.D. from the University of Chicago in 1924, and taught at that University for four years, and at the University of Southern California for twenty-five years. His wife, Pauline V. Young, has also been an Active member of the Society and a Professor at the same University for many years, and is well known for her publications in sociology.

The Review has received news of the death in October, 1952, of Herman Weinheimer, a member of the Society since November, 1951. Dr. Weinheimer was Executive Director of the Jewish Community Service Society in Buffalo, New York. He received the J.D. degree in 1930 from the University of Wurzburg, and the M.S.W. degree from the University of Pennsylvania.

## BOOK REVIEWS



*Social Psychology, An Interdisciplinary Approach.* By HUBERT BONNER. New York: American Book Company, 1953. Preface, 439 pp. \$4.25.

The search for integration, which has been a preoccupation of some social scientists for a decade or two, is undertaken again in this book. Its measure of success, no doubt, will be disputed but it seems safe to predict that it will be widely used and appreciated.

Described as an "interdisciplinary approach" (preferably to behavior and *not* to social psychology), the book has been in preparation for about twenty years which, perhaps, helps to account for the influences acknowledged by the author. These are the field-theoretical views of Lewin, the writings of Cooley and G. H. Mead on the "self," and the approach of cultural anthropology represented by the writings of Margaret Mead. In addition there is considerable evidence of the influence of psychiatric and psychoanalytic theories and concepts.

In the Preface it is stated that the "chief problem of social psychology is the study of the behavior of the individual in the group," and that the aim of the book is to create in the reader "an image of the person-in-his-group, rather than the person as a developing structure, unique and independent." These views are adhered to quite consistently throughout the book.

Part One reviews the historical methodological foundations of social psychology. Part Two relates individual behavior to a matrix of social interaction in such chapters as "Language and Behavior," "The Self and Its Involvements," and "The Motivation of Behavior." Part Three relates individual behavior to cultural influences in such chapters as "Cultural Norms and Behavior," "Ethos and Basic Personality," and "Class Status, Occupation, and Behavior." Part Four is concerned with the behavior of the group as a group, and includes material on prejudice, industrial conflict, and mass behavior. Finally, Part V deals with the plight of the individual in the unstable world of the twentieth century, viewing him not merely as a passive victim of cosmic forces but also as a self-determining agent.

Although this book includes more material

on sexual behavior than is usual in a text, it should be widely used as such. It is concerned with important problems of human life, is based on wide reading and careful scholarship, and is eminently readable.

Amid much that is familiar, the professional student of behavior will also find much that is valuable. The chapter on motivation, for example, strikes this reviewer as an extremely stimulating analysis of a complex subject. And perhaps more of us should reexamine, as does the author, the relative importance of such concepts as "attitude" and "personality" in the slowly emerging science of behavior.

At the same time some dubious aspects of the book should be mentioned. The section purporting to deal with the development of social psychology seems highly arbitrary and one is dismayed at the sudden jump from Wundt to contemporary developments. Some sections are marked by sweeping generalizations about group behavior that the hard-boiled sociologist will reject—though perhaps this is unavoidable in a textbook. Finally, there is a rather uncritical use of the "basic personality" approach which is at variance with the general tendency of the author. However, these are minor reservations to an otherwise highly favorable judgment.

EDMUND H. VOLKART

*Yale University*

*Methods in Social Research.* By WILLIAM J. GOODE and PAUL K. HATT. New York: McGraw-Hill Book Company, Inc. 1952. vii, 386 pp. \$5.50.

The book under review is an elementary undergraduate text in the tradition of Lungberg (1942), Young (1949) and Jahoda, et al., (1951). The authors believe that the "basic logic and research procedures" of modern sociology should be made understandable to the undergraduate student whether or not he wishes to become an active researcher. Mindful of this general education objective as well as their technical responsibility, Goode and Hatt have judiciously woven into the text concrete field materials and their opinions on a variety of methodological questions.

The authors have been refreshingly straightforward in dispatching sacred cows. Take the

question, Is sociology to be a natural or social science? The student is informed that this historical controversy arose before it was realized that sociology, like all sciences, must have concepts, theory, and observations. The quantitative-qualitative dichotomy is rejected with equal directness (p. 313), "The application of mathematics to sociology does not insure rigor of proof, any more than the use of 'insight' guarantees the significance of the research." On the relation of sociology to other social behavior disciplines, the authors neither show enthusiasm for interdisciplinary work as an end in itself, nor any tendency to avoid a mode of investigation which can be competently handled even though it may have been historically associated with another field. On the possibility of a scientific ethic (p. 27), "Science . . . can only tell us *how* to achieve goals; it can never tell us *what* goals should be sought." The writers' general methodological position is one which currently enjoys wide acceptance.

The sixty pages dealing with questionnaires and interviews deserve wide reading. The illustrative material is particularly apt.

The chapters treating of use of the library and preparing the report repeat without any distinctive additions materials which are better a part of a freshman English course. The chapters devoted solely to population methods and scaling suggest that comparable chapters on content analysis, small group experimentation, and similar topics might have with equal propriety been included. The authors do not have Guilford's (1936) knack of talking about analysis methods, which encourages the use of their text as a "cook book." Throughout, the tone is didactic and there is a disposition to tell the student what the best method is rather than to suggest the experimental evaluation of alternative methods.

It is apparent from the organization of the text that the student is not presumed to have had an introductory course in statistics. To compensate for this the authors have inserted various capsules relating to statistics. Erroneous impressions are frequently created by the hurried, compressed treatment. For example on page 227 the notion of the area within  $\pm k\sigma$  of the mean is introduced as valid "since it is known that the distribution in the table is fairly close to the probability curve." Lower on the same page in the discussion of the distribution of sample means, the student is not informed that he may relax the requirement that the underlying distribution be normal. One might guess that this partial information is more damaging to research effectiveness than no information at

all. An equally cruel blow in the cause of non-science is struck on page 233 where in the introduction of scaling the authors state, "Furthermore, attributes are not amenable to mathematical manipulation."

As one reads the two chapters treating of design, the reaction is mixed. The review of Mill and the restatement of Stouffer's fine paper on the four-fold table will in all probability be exciting to introductory students. The exposition stops short of the analysis of results. Because of this the student does not get the added reinforcement which would come from actually selecting the more efficient of two alternative designs. On the other hand the discussion of design in terms of efficiency involves knowledge of statistical hypotheses, types of error, and gains from stratification which imply a statistics course. If in fact, as this analysis suggests, methods are less fruitfully taught if not preceded by statistics, should we not reorder our undergraduate curriculum so that the methods course can assume one semester of statistics? For the schools in which this has been done, substantial portions of the present text will not be satisfactory, but whenever a choice is to be made from the competitive books now available, Goode and Hatt deserves serious consideration.

FRED L. STRODTBECK

*Yale University*

*Twixt the Cup and the Lip; Psychological and Socio-Cultural Factors Affecting Food Habits.* By MARGARET CUSSLER & MARY L. DE GIVE. New York: Twayne Publishers, 1952. pp. 262. \$3.95.

In the late thirties, largely under the enthusiastic sponsorship of M. L. Wilson, then of the U. S. Dept. of Agriculture, a small group of social scientists began a systematic study of American food habits. This work reached a peak in the establishment of the National Research Council's Committee on Food Habits, which functioned efficiently and furiously under the leadership of Margaret Mead throughout the war. The social scientific study of food habits does not appear to have survived the late conflict, and it is to the considerable nostalgia and delight of the reviewer that this book, representing one of the two or three major field research ventures, has finally appeared. (One of the others, done by Herbert Passin and the reviewer in southern Illinois, has seen publication in the form of a few scattered articles only—two in the *Review*.)

The book is a compromise between a technical monograph and a popular presentation.



Effort has been made to keep social scientific jargon and interpretations in the wings, where they can be summoned forth when needed. But in spite of this somewhat studied effort, the book reads well and the major findings come clear. Both authors have avoided continuous academic careers, and are engaged in a wide variety of public relational and mass communication ventures. On the whole the sprightly atmosphere introduced by this background adds to the volume rather than detracts. There is some natural difficulty in maintaining a consistently profound and serious scientific note when the major subject of attention is simple material object (food) and a large number of complex human and social phenomena are held against it, as it were.

The data organized and synthesized in this book is drawn from a series of studies made in 1940-41, in different parts of the rural Southeast, the different regions being chosen for their variation in economic level and ethnic background. The materials are not presented as community studies or field monographs, but have been thoroughly worked over and offered as specimen findings of general import. Thus the authors move from the individual and his tastes, to the problem of social transmission and culture change in "foodways," winding up in food as an indicator of social status and relational patterns. A concluding section deals with recommendations and implications for food policy. The major theme in this last is the "unreason" of food policy planners in ignoring the psychology of preference and the status value of food—or, in some cases, in the "sentimental" over-recognition of these. Everything that is said is good, but the slightly racy style, the almost complete omission of questions about food in the context of power, and a conceptual approach which fails to close in on the vital questions of food as an institutionalized means to need-satisfaction, leaves the reader somewhat dissatisfied. The social science of food is more than culture, tradition, and social status—it is also politics, power, strategy, means and ends, human needs.

Cultural anthropologists will find considerable interest in those chapters dealing with food as a cultural trait, which is selected, modified, and transmitted in the course of the social process. This section amounts to a respectable empirical contribution to the theory and documentation of culture (not to mention to the history of early America). Sociologists will be most concerned with those chapters and sections dealing with food as an article of interpersonal interaction and status symbol. While the available comparative evidence is slight, largely due to the failure of the work

of the reviewer and others to see full publication, it may be added that in virtually every detail the Cussler-De Give findings are paralleled in the other data and interpretations secured. The socio-cultural dynamics of food in America—at least in the standard descriptive framework of culture history, patterning, and social status—is now on record with the publication of this most interesting volume.

JOHN W. BENNETT

*Ohio State University*

*An Introduction to Anthropology.* By RALPH L. BEALS and HARRY HOIJER. New York: The Macmillan Company, 1953. xxi, 658 pp. \$6.00.

Professor Beals and Hoijer of the University of California, Los Angeles, have long been interested in the development of generalized anthropological-sociological theory. This text is appropriate for conjoint, interdisciplinary departments in both fields and especially useful for courses in socio-cultural and physical anthropology. Because of its broad scope and aim, the book begins with man's place in nature, incorporating new and non-controversial material on human evolution and genetics, and race, and continues with the socio-cultural universe down to applications of modern anthropology. It is, however, more than a text, being both systematic and self-consistent in theoretical development. At the same time, it has several advantages as a text: logical, simple and direct development of topics; success in relating the material, social, religious, political and artistic areas of culture in an integrated and truly functional manner; a rounding out with added chapters on Culture Change, Culture and Personality, Acculturation and Applied Anthropology—so that a student can consider problems in dynamics; and an avoidance of empty polemics, but never of issues of importance bearing upon the direction of social science. Throughout, the data on evolution or the art of a tribe and nation, etc., are skillfully illustrated by Dr. Virginia More Roediger. Excellent choices in collateral reading and ethnographic bibliographies are included. In short, it is an excellent text and one of the better books in social science of this decade.

While the authors are aware that cultural phenomena may be studied in New York or Paris, as among Eskimos and Hottentots, they feel that the data from such diverse sources can be developed in a unitary theory. Theirs is primarily, then, a systematic treatment of socio-economic and cultural organization. Chapters on economics and family, language and the arts, and on socio-political organization

are among the better brief treatments in the literature. If the reviewer has any criticisms, these relate to two sections, both of value, but so incomplete as to be useless in a systematic general theory of culture. In the chapter on marriage, definitions run away with development; for the marital forms have not, we think, been carefully related to the total style of interpersonal relations in the societies studied, text-wise, and chosen for examples. Also, while the brief discussion of education is all to the good, the larger implications of personality formation are dealt with in a manner that seems naive from the psychiatric point of view. Regarding the latter topic, of such interest today, the authors themselves state that their section is provisional and temporary. It is obvious that their interests in cultural dynamics have illuminated all aspects of the book relating to this topic.

Yet the way in which cultural dynamics or even cultural typologies relate to the psychology of culture (or culture-personality interrelations) is little explored except through a somewhat mechanical and non-dynamic learning theory, along with some strictures on psychoanalysis, some not too clear references to field theory, and some not very colorful summaries of some ethnologies. The psychology of art, as of language, can be explored. The interrelations of a *cultural* anthropology and a *social* psychiatry lie at the heart of contemporary social science theory and thinking. Perhaps simplifications are intended in an introductory work. Even with these strictures on two or three sections, it is the best book of its type in years.

MARVIN K. OPLER

Cornell University Medical College  
Department of Psychiatry

## BOOK NOTES

*An Introduction to Social Science.* Selected, written, and edited by ARTHUR NAFTALIN, BENJAMIN N. NELSON, MULFORD Q. SIBLEY, DONALD C. CALHOUN, with the assistance of ANDREAS G. PAPANDREOU. Chicago: J. B. Lippincott Company, 1953. xvii, 1129, xxx pp.

The late Stephen Leacock once remarked that he "didn't see why encyclopedias should be so heavy, since they weren't sold by the pound." This encyclopedic anthology will very likely give satisfaction if appraised by either use of the adjective. Its three pounds and 1176 pages encompass 134 well-selected readings which are heavy enough in content to satisfy the scholar and light enough in presentation to beguile the student.

The book is the product of the introductory social science course in the Department of General Studies at the University of Minnesota, and is designed to provide the core of reading for a year-long course. It is divided into three parts, each integrated around one of three central themes: personality, work, and community.

The range of authors is truly wide; the editors have not confined their selections to the social scientists but rather have sought out authors who have had something to say about the knowledge with which social science deals. Thus, in addition to social scientists, poets and statesmen, jurists and philosophers have been drawn upon. The unity of the whole has not been left to the implications of content,

but has been seen to by comprehensive introductory material. In each of the major sections, the problems of the areas of inquiry have been outlined, contributions to their solution presented, and the involved values reviewed. References and bibliographical materials are abundant.

Whatever value there may be in survey courses—and their increasing adoption in the major universities implies that much store is set by them—inheres in this thoughtfully edited volume.—FREDERIC W. TERRIEN.

*Reader in Public Opinion and Communication.* (Enlarged Edition) Edited by BERNARD BERELSON and MORRIS JANOWITZ. Glencoe, Illinois: The Free Press, 1953. xi, 611 pp. \$5.50.

This enlarged edition of the successful and widely used source book adds some ten papers on methods in public opinion research to the forty or more selections which range from Lowell's classic statement of the nature of public opinion, to a section of a Federal Communications Commission report on the responsibility of broadcast licensees. The volume has more than demonstrated its usefulness not only as an instrument for teaching, but also simply as a convenient collection of source materials. This is the first of the Free Press reader series to be issued in a new edition. Both the publisher and the editors are to be congratulated.

In the opinion of this reviewer the title would be somewhat more explicit if it were lengthened by one word and one letter, viz. "Reader in Public Opinion and *Mass Communications*." Such an alteration would justify the omission of any of the more recent work in the field of interpersonal communication. As it is, the volume in this respect, rather surprisingly, starts and stops with Mead. But possibly such materials belong in another reader on general communication theory.

The concluding item of the new readings on methods is drawn from a 1948 paper by Blumer which calls for a more sociological approach to public opinion. This essay, perhaps unfortunately, stimulates the reader to look for some mention of the more recent developments in the field. Although the past two or three years have been quite productive both methodologically and substantively, one can find no reference to any of the work published since 1950 (the date of the original edition). Nor has the Bibliography been brought up to date. (It continues to comment apologetically for the lack of materials on method.) Finally, it is perhaps unnecessary to comment on the lack of an index. Such a useful book as this, however, deserves every publishing amenity and encouragement to use.—  
JOHN W. RILEY, JR.

*Criminology: A Book of Readings.* By CLYDE B. VEDDER, SAMUEL KOEING and ROBERT E. CLARK. New York: The Dryden Press, 1953. xxi, 714 pp. \$4.50.

This book of readings is intended to give students taking courses in criminology some opportunity to become acquainted with the more important writings of some of the past as well as contemporary criminologists. It makes readily available to both the teachers and students of criminology a great deal of essential materials to supplement those found in the usual textbooks. In so doing it meets a need and it should prove to be quite useful to both teacher and students.

The selections included cover the the topics usually presented in most textbooks in criminology. The readings, therefore, may be easily tied to and make part of several more recent textbooks (Sutherland, Reckless, Gillin, Taft, Barnes and Teeters, Cavan, Elliott, and von Hentig).

The enthusiasm with which this book is received will, to a very great extent, depend upon the reader's general orientation. The authors have however, been quite successful in bringing together a rather adequate sample of the thinking on crime and the criminal.

This is especially true for the part devoted to factors in criminality.

Each selection is introduced by a short paragraph which indicates what the reading is about and contains a brief notation on the author. No attempt is made by the authors (editors) to tie the different sections together.—  
ELIO D. MONACHESI.

*Negroes in the United States: Their Employment and Economic Status.* By Bureau of Labor Statistics, United States Department of Labor. Washington, D.C.: U.S. Government Printing Office, 1952. vi, 58 pp. Thirty cents.

This report describes recent national trends, primarily those of the 1940–1950 period, in the economic and employment status of Negro men and women in relation to that of whites. Background data are supplied in regard to growth in population, birth and death rates, and life expectancy, for the period 1900–1950, significant population changes from 1940 to 1950, and educational status, 1940 and 1950. In addition to trends involving Negroes and whites in the labor force, industry trends in employment, and occupational trends, 1940 to 1950, information is supplied concerning placement experience of public employment offices (1940, 1944, and 1951), duration of employment on current jobs (January, 1951), work-life expectancy (1940), income of families (1945–1950); wage and salary income (1939 and 1947–1950), and insurance status and wage credits under the Old-Age and Survivors Insurance program (1937–1949). The text is supplemented by twenty charts and twenty-six tables. As stated in the "Letter of Transmittal" (p. ii), "two general facts emerge. The first is that in almost every significant economic and social characteristic that we can measure . . . our Negro citizens, as a whole, are less well-off than our white citizens. The second is that in almost every characteristic the difference between the two groups have narrowed in recent years."—CLARENCE E. GLICK.

*Encyclopedia of Aberrations: A Psychiatric Handbook.* Edited by EDWARD PODOLSKY, M.D. New York: Philosophical Library, 1953. viii, 550 pp. \$10.00.

This volume will make a useful addition to any library of psychiatric literature but it does not fulfill the claims of the foreword or the title. Approximately four-fifths of the volume consists of articles from various psychiatric journals and the *Handbook of Correctional Psychology*. The remaining articles



were presumably prepared especially for this volume. In the following respects it falls short of being encyclopedic. (1) Many topics of some psychiatric consequence, e.g., hypnosis, war neuroses, involuntal melancholia, neurasthenia, receive no separate treatment, although they receive incidental mention in other articles. (2) There are neither index nor cross references to facilitate the exploitation of the encyclopedic potentialities of the volume. (3) Except in the form of footnotes to previously published articles, there are no bibliographical references to the literature. (4) The allocation of space is, at the least, haphazard. "Castration complex" receives 4 lines; "Genesis of Homosexuality," 7 columns; "Fear of Post-orgastic Emptiness," 19 columns; and 3 articles on body-image disturbances, 66 columns. (5) Few of the journal articles, the greater bulk of the volume, were ever intended to be encyclopedic in nature. Thus, there is a 36-column article on "Manic-Depressive Psychosis (Mild), Depressive Type, Psychiatric and Clinical Significance," but there is no general article on manic-depressive psychosis. (6) There is relatively little representation of non-psychanalytic points of view.

To give the book its due, it remains a valuable collection of papers ranging, although somewhat desultorily, over the whole field of psychiatry. Many of these papers are of the highest quality and of value and interest to the social scientist as well as the psychiatrist. —ALBERT K. COHEN.

*Retirement and the Industrial Worker: Prospect and Reality.* By JACOB TUCKMAN and IRVING LORGE. New York: Bureau of Publications, Teachers College, Columbia University, 1953. xvi, 105 pp. \$2.75.

This is a report on an investigation of attitudes toward retirement of unionized workers in the needle trades. These attitudes were studied under consideration of possible variations among three groups: (1) workers who were not yet retired; (2) workers in process of retirement; and (3) workers retired on a pension. The study was undertaken as a co-operative project by the New York Cloak Joint Board of the International Ladies' Garment Workers' Union, the Federal Security Agency and Teachers College, Columbia University.

The study deserves special attention because it refers to workers for whom retirement is not compulsory. Thus it makes a contribution to the evaluation of a type of retirement which has been proposed as a remedy for the disadvantages created by compulsory retirement at a specific age.

Some of the findings reported by the authors contradict widely held assumptions. For instance, retirement tends to look less attractive as the worker grows older; the "reported" health status of the retired respondents was "contrary to the belief that retirement results in a rapid physical decline and in early death"; and children were reported to be more favorable to the retirement of respondents than their wives. If these findings should be corroborated by the results of further studies, our thinking about adjustment to retirement may need considerable correction.

The report concludes with a number of recommendations which representatives of government, management, and labor may find useful stimuli for their thinking on questions created by retirement.

In essence this report, although hampered by the typical handicaps of presenting the quantitative aspects of an attitude study, manages to convey information which every student of retirement in our industrial society will want to know and to consider.—OTTO POLLAK.

*Neighbors or Urbanites?: The Study of a Rochester Residential District.* By DONALD L. FOLEY. Rochester, N.Y.: Department of Sociology, University of Rochester, 1952. 75 pp. \$1.00. Mimeographed.

This study in the field of urban community organization deals with the problem of the relative orientation of metropolitan population toward local residential areas in one direction and toward the specialized agencies of the community at large in the other. The specific empirical data, gathered from a sample survey of fifty blocks in Rochester, covers (1) the extent to which residents use local facilities; (2) the extent of informal neighboring practices; and (3) the extent to which the residents identify the local area as a "community." These three variables were hypothesized at the outset to be subdimensions of an over-all neighborhood-urban continuum. Analysis of the data revealed, however, that these hypothesized components had low intercorrelations and, therefore, may be separate dimensions.

The general conclusion of the work is that, while the activities of metropolitan residents have become segmentalized and diffused over the whole community, appreciable neighborhood orientation persists. The distinctive population and cultural characteristics associated with the different forms of local orientation are indicated in the report.

The study is creditable, first, for the manner in which its problem is placed into the perspective of the general theory of urban structure; second, for the way it relates to earlier

studies and suggests further research; and, thirdly, for its straightforward and effective methodology.—JOSEPH COHEN

*A Prospect of Cities: Being Studies Toward A History of Town Planning.* By CECIL STEWART. New York: Longmans, Green and Company, 1952. xvi, 191 pp. \$4.50.

As the subtitle indicates, this book contains a series of essays on various conceptions of the city plan that have obtained at different times and places. Beginning with some of the earliest notions of planning developed by the Greeks in the fifth century, B.C., the author passes on to an examination of the plans of Roman outpost towns, Constantinople, and British and Norman villages in the twelfth and thirteenth centuries. A discussion of the utopian schemes of the Renaissance is a preface to a consideration of the efforts of the French to give physical embodiment to the earliest intellectual treatment of city design. Richelieu and Versailles are presented as prototypes of the post-Renaissance formalism. The model of Versailles, transplanted to England, underwent a metamorphosis with the rise of industrialism. And in the eighteenth and nineteenth centuries the garden city movement appeared as a solution to the congested and unsanitary agglomerations produced by the factory system. In each instance the author searches for the genesis of planning ideas in the social, political, and economic context.—A. H. HAWLEY.

*An Approach to Urban Planning.* Edited by GERALD BREESE and DOROTHY E. WHITEMAN. Princeton: Princeton University Press, 1953. ix, 147 pp. \$2.00.

The phenomenal growth of new urban areas in the outlying parts of our cities alone should challenge the ingenuity of the practitioners who have to provide the physical prerequisites of urban life. When linked together with the obsolescence and decay of central city areas the planning task takes on formidable proportions.

Therefore, it is of considerable interest to students of urban life to learn how some of the foremost planners in this country view the assignment of rehabilitating obsolescent areas and of guiding the growth of new urban neighborhoods in our metropolitan areas. That such a task cannot be accomplished without the full and conscious participation of the citizens is the theme of Hugh R. Pomeroy's article "The Planning Process and Public Participation." He traces the history of the planning movement in the United States and describes the various types of civic organizations which are supporting planning activities

of one kind or another. A description of the proper functions of citizen participation in the planning process concludes his contribution. The close relationship between the physical shape of the city and the mode of transportation is sketched by C. McKim Norton in his paper "Metropolitan Transportation." This paper traces the difficulties in the central areas as well as the suburbs to the ever increasing use of the automobile. According to Norton, the only sound transportation system in an urban area is mass transport by rail, bus, or both. He, therefore, questions the soundness of developing a system of urban highways which chokes the central business district and competes with public transportation. Norman Williams, Jr., in his article "Land Use and Zoning" examines the major types of land uses and describes the emergence of new use areas such as truck terminals and garden factories. In addition, he evaluates the effectiveness of zoning regulations. In "Housing and Urban Redevelopment," Charles K. Agle analyzes various aspects of the urban house and examines the opportunities of redevelopment under federal aid. Finally, Walter H. Blucher in "Fiscal Programming" emphasizes the fact that financial planning is an integral part of the total planning process. The annotated bibliography at the end of the book deserves special mention. This little volume furnishes a good base for a deeper social and psychological understanding of city life, mid-century style.—ERICH ROSENTHAL.

*Tomorrow's Chicago.* By ARTHUR HILLMAN and ROBERT J. CASEY. Chicago: The University of Chicago Press, 1953. x, 182 pp. 3.50.

A sociologist and a journalist have collaborated on a book designed to inspire the citizens of Chicago to strive for a better community. The volume is not intended to make any contribution to science. Its sole concern is enlisting the support of the reader for the development and application of a comprehensive master plan for the Chicago metropolitan area.

The authors begin with a popular account of the factors which have brought Chicago to its present condition, emphasizing the importance of planning and lack of planning in developing the present structure of the city. Some current problems are then briefly stated, with general suggestions for their solution. The final chapters are devoted to a plea for planning and the involvement of individual citizens in the planning process.

From a sociological standpoint, the major deficiency is a tendency to ignore the existence

of interest groups which oppose the program of the planners. The omission of such material is probably intentional, since the authors are attempting to arouse the enthusiasm of the general public.—SANFORD M. DORNBUSCH.

*Utopia Ltd: The Story of the English New Town of Stevenage.* By HAROLD ORLANS. New Haven: Yale University Press, 1953. xvi, 313 pp. \$4.50.

That the garden city ideal is not reached at a single bound is amply documented in this case study of the struggles to build a new satellite city some 30 miles north of London. By depicting the politico-social setting within which an array of town planners and administrators sought to develop Stevenage, the author makes a distinctive contribution to the sociology of planning.

Under the New Towns Act of 1946, the Stevenage site was selected as the forerunner of eight decentralized industrial cities in the Greater London area. It was proposed to

transform the existing town of 6,000 to a thriving new community ten times that size within the decade. The completion of only 28 houses in the first four-and-a-half years symbolizes the obstacles that the Stevenage Corporation and the Ministry of Town and Country Planning encountered.

The rather great detail with which the book treats a number of the development problems somewhat detracts from an otherwise very readable style. This is generally offset, however, by the quality of the "critical analysis" (to use the author's own phrase) that Dr. Orlans is able to make. As a nonplanner he achieves a broad view of the planning process. There are, he concludes, three major problems suggested by the Stevenage experience: the conflict between local community and national interests, "the extent to which efficient executive action is compatible with the democratic process," and the inevitably authoritarian tone of a utopian scheme.—DONALD L. FOLEY.

## PUBLICATIONS RECEIVED

(Listing of a publication below does not preclude its subsequent review)

(AMERICAN JEWISH CONGRESS AND NATIONAL ASSOCIATION FOR THE ADVANCEMENT OF COLORED PEOPLE). *Civil Rights in the United States, 1952: A Balance Sheet of Group Relations.* New York, 1953. 151 pp. 40 cents.

ANDERSON, OSCAR EDWARD JR. *Refrigeration in America: A History of a New Technology and Its Impact.* Princeton, New Jersey: Princeton University Press, 1953. ix, 344 pp. \$6.00.

BAKER, W. P. *The English Village.* New York: Oxford University Press, 1953. 224 pp. \$2.00.

BARKER, ROGER G. (in collaboration with BEATRICE A. WRIGHT, LEE MEYERSON AND MOLLIE R. GONICK). *Adjustment to Physical Handicap and Illness: A Survey of the Social Psychology of Physique and Disability.* (Bulletin 55, Revised). New York: Social Science Research Council, 1953. xvi, 440 pp. \$2.00.

BEALS, RALPH and HARRY HOIJER. *An Introduction to Anthropology.* New York: The Macmillan Company, 1953. xxi, 658 pp. \$6.00.

BOWEN, E. R. *The Cooperative Road to Abundance: The Alternative to Monopolism and Communism.* New York: Henry Schuman, Inc., Publishers, 1953. xiii, 169 pp. \$3.00.

BOYLE, GEORGE. *Father Tompkins of Nova Scotia.* New York: P. J. Kenedy and Sons, 1953. xi, 234 pp. \$3.00.

BUNZEL, RUTH. *Chichicastenango: A Guatemalan Village* (American Ethnological Society Publication, Vol. XXII). Locust Valley, New York: J. J. Augustin, Inc., 1953. xxvi, 438 pp. \$7.00.

BURGESS, ERNEST W. AND PAUL WALLIN. *Engagement and Marriage.* Chicago: J. B. Lippincott Co., 1953. xii, 819 pp. No price indicated.

(CALIFORNIA DEPARTMENT OF MENTAL HYGIENE AND THE LANGLEY PORTER CLINIC). *California Sexual Deviation Research.* California State Printing Office, 1953. 156 pp. No price indicated.

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